Basic Research in the Navy

Volume I

Report to Secretary of the Navy by the Naval Research Advisory Committee

FC

Volume I

A Report to the

Secretary of the Navy

on

Basic Research in the Navy

by the

Naval Research Advisory Committee

June 1, 1959

Table of Contents

Members of The Naval Research Advisory Committee

Dr. R. F. Bacher, Chairman, Physics Dept., California Institute of Technology

Dr. C. C. Furnas, Chancellor, University of Buffalo

Dr. T. K. Glennan, Administrator, National Aeronautics and Space Administration

Mr. E. H Heinemann, Vice President, Douglas Aircraft Company, Inc.

Dr. R. A. Kern, Temple University Hospital

Dr. A. B. Kinzel, Vice President, Union Carbide Corp.

Dr. J. W. McRae, Vice President, American Telephone and Telegraph Company

Mr. G. Norton, President, Institute for Defense Analysis

Dr. E. R. Piore, Director of Research, International Business Machines Corp.

Dr. I. I. Rabi, Department of Physics, Columbia University

Dr. R. Revelle, Director, Scripps Institution of Oceanography

Dr. F. Seitz, Chairman, Physics Dept., University of Illinois

Dr. C. G. Suits (Chairman, NRAC) Vice President and Director of Research, General Electric Company

Dr. F. E. Terman, Provost Stanford University

Dr. E. A. Walker, President, Pennsylvania State University

DEPARTMENT OF THE NAVY
NAVAL RESEARCH ADVISORY COMMITTEE
WASHINGTON 25, D. C.

IN REPLY REFER TO:

ONR:103:jg
Ser N-152
24 Apr 1959

My dear Mr. Gates:

The report transmitted herewith for your consideration marks a beginning of research on research in the Navy. We are fully aware that without development, production and operational training, there can be no effective fighting force. However, the current thinking with respect to research, and especially basic research as a Naval requirement, is much less clear and the relationships in this area have not been fully developed. This report begins to lay the basis for a clear expression of the requirement, bearing in mind that the success of the Navy in accomplishing its mission in competition with other world powers depends largely on a continuous flow of new and better weapons and techniques. This in turn, requires the continuous development of new technologies which have their roots in the results of basic research.

The report strongly supports the Navy's need for basic research. Only by active participation in a program for which it assumes a direct responsibility can the Navy insure a rapid flow of the products of new science from the laboratories of the Nation into the uses of the Service.

The Naval Research Advisory Committee believes that this report makes an appreciable contribution to a development of the understanding of the relationship of basic research to the missions of the Navy. However, we are acutely aware of many unsolved problems and we hope this report will provide the basis for further study.

The Committee urges that the Navy implement the recommendations of the Naval Research Advisory Committee, herewith presented.

Very truly yours,

C. G. Suits

C. G. SUITS, Chairman
Naval Research Advisory Committee

Honorable Thomas S. Gates, Jr.
Secretary of the Navy
Washington 25, D. C.

THE SECRETARY OF THE NAVY
WASHINGTON

24 JUN 1959

From: Secretary of the Navy
To: Chairman, Naval Research Advisory Committee

Subj: Report on "Basic Research in the Navy"

1. Having reviewed the Naval Research Advisory Committee report on basic research in the Navy, I would like to take this opportunity to congratulate the members of the Committee for their thorough and constructive analysis of the problem of basic research in the Navy. This analysis will be an important management aid in the proper administration of naval research programs.

2. The recommendations contained in the report will be very seriously considered and will be invaluable in our budgetary deliberations. I am sure, however, that the Committee is aware of the dangers which would attend fixing any part of the budget at an arbitrary percentage. I appreciate the opinions that basic research should be favored at this stage in our national affairs. At the same time, we must realize that our extensive national commitments require great care in maintaining a balance between the various portions of the total budget.

3. May I express the appreciation of the Department of the Navy for this pioneering cooperative effort. The special care and deliberation that has gone into the conclusions and recommendations of the Committee is apparent and recognized. The Naval Research Advisory Committee has rendered most effective and valuable service by producing this report.

W. B. Franke

Conclusions and Recommendations

of the
Naval Research Advisory Committee
concerning the report "Basic Research in the Navy"

This report sets forth the nature of basic research and its relationship to military end items. It establishes, by historical example and otherwise, the Navy's need for an increasing flow of basic research.

Basic research has played a tremendous role in the past, transfiguring the Navy by findings in such fields as radar, inertial guidance, missile propulsion, and atomic propulsion, and the accelerated pace of scientific progress in the last decade emphasizes its importance. The report points out that while the Navy can support only a small part of the total research of the world or the country, it must do enough in each area of interest to provide effective coupling and judgment for its own needs. It must also do that basic research essential to provide for its own direct needs in those areas of peculiar interest to the Navy which are not being adequately covered elsewhere.

In conducting basic research for either of these reasons, the investigators within the Navy Department must be constantly alert to recognize the impact of any findings on the needs of the Navy Department. These may not necessarily be related to the immediate objective of a given project but may well bear on the potential over-all position of the Navy. This is truly important. Time and time again, as brought out in the report, unexpected or even incidental findings have resulted in a major improvement in weaponry, communications, and the like. Said another way, only those engaged in basic research in a given area who, at the same time, have Navy interests at heart, are in a position to appreciate scientific findings of others and the significance of such findings to the Navy.

The report sets forth the judgment of those engaged in the direction and application of basic research in industry with respect to the level of basic research appropriate to the total Navy effort. Essentially this judgment is to the effect that the basic research effort in the Navy be approximately doubled in order to restore the former relationship of basic research to the total research and development effort. This would

also bring the proportionate Navy basic research effort closer to that now current in those progressive industries operating in the areas of science and engineering.

The Committee concurs with the findings of the Arthur D. Little Study Group. It believes that this study lays the basis for detailed consideration of the basic research program required to fulfill the Navy's needs. However, it should be emphasized that this laying of the groundwork is but the first step in the process of rehabilitating the Navy's basic research program. In order to implement such rehabilitation a second step should be pursued forthwith.

The next step comprises the detailing of the program proper. Study of such detailing can be done well only by those who have a close working relationship in the Navy and with the scientific community, namely, the Office of Naval Research. It is recommended that this group prepare detailed programs in each of the fields of science related to the missions of the Navy as set forth on Page 49 of the report, plus such others as may be pertinent. In considering these fields it is obvious that certain items are the prime responsibility of the Navy; for example, oceanography. It is obvious that others are a major responsibility of the Navy; for example, meteorology, navigational phases of astronomy and astrophysics, marine phases of biology and biological sciences, the claustrophobic phase of psychology, and the like. Other areas are so broad that they are found wherever basic research is being done; for example, physics, material sciences, mechanics, electronics, mathematics, and the like. In these areas an effort sufficiently large to provide good coupling is needed. By setting forth specific programs pertinent and suitable to each of the areas in question and bearing in mind the foregoing, an over-all program can be prepared.

The approach just outlined is by no means novel, having been attempted more than once in the past. These attempts have not borne fruit because they consistently showed a requirement for total funds many times greater than contemplated at the time, and the principle of selection by areas was abandoned in favor of priority projects. To prevent this, after such a total program has been prepared by assembling detailed projects, a third step is in order. There must be another critical review still following the area distribution to bring the total cost within the augmented budget. If the budget augmentation is sufficient, i.e., double that of fiscal 1959, as herein recommended, the over-all program should approach the fulfillment of the needs herein set forth. Experience with the augmented program will show the success of the proposed approach and additional steps may be taken in future years, as necessary.

It is the Committee's recommendation that ONR proceed immediately with the studies outlined above and that a program corresponding to a doubled budget be prepared by the Office of Naval Research and be endorsed by the Secretary of the Navy.

Acknowledgments
By
The Naval Research Advisory Committee

The Naval Research Advisory Committee is pleased to acknowledge the capable efforts of Arthur D. Little, Inc., in the pursuit of a study of Basic Research in the Navy and the preparation of this report of the study under the direction of Vice President, Project Director, Dr. Bruce Old and his associates. In addition, we acknowledge the able work of the Navy's Training Device Center for the creation of the excellent graphics contained in the report. We particularly appreciate the effective work of the following deputies of the NRAC members who have provided continuous liaison and critical appraisal of the progress of the study for the Committee:

Mr. R. W. Larson	(for C. G. Suits)
Mr. G. M. Morrow	(for A. B. Kinzel)

As well as the able assistance of:

Mr. W. H. Doherty	(for M. J. Kelly*)
Mr. L. A. Cookman	(for E. R. Piore)

* Dr. Mervin Kelly was a member of NRAC at the time this study was undertaken, and he and his deputy contributed much helpful counsel and advice to the progress of the work. His appointment terminated prior to the completion of this report.

Volume I

A Report to the

Naval Research Advisory Committee

on

Basic Research in the Navy

June 1, 1959

Report Prepared
by

Arthur D. Little, Inc.

UNDER OFFICE OF NAVAL RESEARCH CONTRACT NO. NONR-2516(00)

Table of Contents

Summary and Findings

During World War II it became strikingly evident that scientific research is essential to the national security. The Scientific Research Board Report to the President in 1947 forcefully emphasized this point, stating:

> "The security of the United States depends today, as never before, upon the rapid extension of scientific knowledge. So important, in fact, has this extension become to our country that it may reasonably be said to be a major factor in national survival."

The Department of the Navy, fully cognizant of this trend, led the Federal Government in implementing changes in its organization and budget to reflect the requirements for expansion in scientific research. With the establishment in 1946 of the Office of Naval Research "to plan, foster, and encourage scientific research in recognition of its paramount importance as related to the maintanence of future naval power, and the preservation of national security," the Navy increased sharply the percent of its budget devoted to research.

Research in science and engineering is generally considered to consist of a continuous spectrum of activity having as its three major segments basic research, applied research, and development. Only by having a properly balanced and administered program at any given time in all segments can the rapid evolution of new weapons systems and techniques of warfare be reasonably assured. The most perplexing problem in achieving a properly balanced research program for the Navy is the establishment of an appropriate level of participation in basic research. There are two major reasons for this. First, there has been some lack of definitive understanding as to the nature of basic research and its role in the furtherance of the missions of the Navy. Second, substantial Government sponsorship of basic research is so recent a factor that policies are still in the formative stage. Therefore, at the recommendation of the Naval Research Advisory Committee, this study was

undertaken to attempt to determine a basis for decision by the Department of the Navy in establishing proper levels of participation in basic research. Despite the obvious difficulty of this assignment, the potential usefulness of any quantitative findings in promoting future Navy effectiveness was thought to make the undertaking worthwhile.

For purposes of this study, the official Department of Defense definition of basic research was utilized. This definition, found to have broad acceptance by industry, university, and Government personnel, is as follows:

> "Basic research is that type of research which is directed toward increase of knowledge in science. It is research where the primary aim of the investigator is a fuller knowledge or understanding of the subject under study." (Ref. DOD 3210. 1 Nov. 12, 1957)

The key question at the outset of this project was whether a necessarily broad definition of this type was interpreted in a sufficiently rigorous manner to permit the nation-wide collection of comparable and valid data on basic research policies, budgets, and expenditures from Government, industry, and university sources. This is a problem which has bothered the Congress and the Bureau of the Budget in the past. Considerable effort was expended in studying this matter, and it is gratifying to be able to report real progress toward clarification of this issue.

the output of all meaningful basic research is almost invariably represented by publication in the form of papers appearing in recognized scientific journals

The output of all meaningful basic research is almost invariably represented by publication in the form of papers appearing in recognized scientific journals. The infrequent cases of secrecy in basic research cause a delay in, but do not prevent, publication. This being true, if there is widespread consistency in the interpretation of what constitutes basic research, a correlation should exist between the number of people claimed to be performing basic research in Government, industry, and university laboratories, and the number of papers originating from each of these sources appearing in selected scientific journals. In the investigation of this assumption, data collected by the National Science Foundation were used to calculate the number of basic research workers claimed by Government, industry, and university laboratories, and the number of papers originating from each source was obtained by inspection of a selected sample of thirteen recognized scientific journals. A sufficiently strong correlation was obtained, between numbers of research workers and numbers of papers, to permit the conclusion that policy with respect to basic research definition and freedom to publish, is remarkably consistent nation-wide. On the basis of this important knowledge, it then became possible to collect with more confidence data from a number of sources for comparison of basic research policies, budgets, and expenditures. Furthermore, it was possible to make simple, rough checks as to reasonable validity of the data.

In the course of this assignment to assist the Navy Department in basic research policy formulation, three lines of attack were pursued:

a. Orientation

It became evident at the outset of the study that a broader understanding of basic research is a necessary step in evolving improved basic research policies. Therefore, much effort was devoted to the development of a concise and novel presentation, as given in this report, of the dependence of the Navy on technology, the nature of basic research, and the relation of basic research to the missions of the Navy.

b. Judgment and Analysis

People skilled in the art of administration of research were sought out in order that their experience and judgment as it might apply to the assignment could be used to advantage. This involved discussions with leaders in industry, in Government, and in universities.

New and extensive data on research and research personnel were collected and analyzed.

c. Quantification

A unique approach was made toward the synthesis of a mathematical model of the relationships between segments of the research process, in an attempt to develop a method for predicting proper levels of effort in each segment of the process.

Principal Findings

participation by the Navy in basic research in many fields is essential to the furtherance of its missions

Careful study has shown that participation by the Navy in basic research in many fields of science is essential to the furtherance of its missions. In this period of accelerating technological advance and dynamic international competition, national survival is largely dependent upon speed of acquisition and application of new knowledge. The vital role of basic research in accelerating progress is clearly demonstrated by a study of actual case histories, presented herein in the form of schematic models, and by an analysis of the research practices of leading corporations similarly faced with the problem of survival in this age of technology.

A dominant requirement of the Navy today is that of leadership in the development of new weapons systems and techniques of warfare in this period when rapid technological advance and international competition combine to render obsolete many weapons even before the production stage can be initiated. Such leadership can be maintained

with participation in basic research, scientists remain constantly abreast of the expanding frontiers of world science

only by means of an aggressive, wisely conceived, properly balanced, and skillfully managed research and development program involving many fields of science. Essential to the success of such a program is effective participation in basic research, the life blood of the entire system of technological innovation. The basic research segment of the program is responsible not only for developing new knowledge, but also for communicating with the frontiers of science on a world-wide basis, and transmitting such knowledge or understanding to closely coupled applied research and development segments in order to maximize its utility. This vital function can be performed efficiently only by scientists actually participating in basic research and familiar with the needs of the Navy. With participation the basic research, scientists remain constantly abreast of the expanding frontiers of world science, and maintain the conceptual ability necessary to assist in evolving rapidly those applications vital to enhanced Navy effectiveness. Without participation, communication slows, the life blood is drained, and the over-all research program quickly deteriorates.

During the decade 1947 to 1957 leading corporations in high technological obsolescence rate industries have been far more aggressive in their participation in basic research than has the Navy.

While the basic research requirements of the Navy cannot be exactly compared with those of any other organization, the best available possibilities for comparison are found in technically based industries. Industry represents the second largest source of basic research funds. Many corporations have endeavored to evolve sound policies with respect to the extent of their participation in basic research in order to achieve that balance in their research and development programs most likely to guarantee corporate growth in the face of stiff competition in a period of accelerating technological advance. Information on research and development expenditures was, therefore, gathered from a number of leading technically based corporations. Excluded from the figures were Government contracts and those engineering activities not normally included in the research and development budget.

In 1947 the Navy allocated 10 percent of its research and development expenditures to basic research. This compared very favorably with the policies of many leading industrial corporations. However, a distinct divergence of policy occurred over the next ten years. Data from two of the most successful corporations in each of five technically based industries (chemical, petroleum, communications-electronic, pharmaceuticals, materials) showed these ten corporations in 1957 devoted 10-20 percent of their own research and development expenditures to basic research. The average allocation of 16 percent is in marked contrast to the Navy which currently allocates only 6-8 percent of its research and development budget to basic research.

Dollar figures add further confirmation. Information supplied by fourteen top corporations in these same industries showed that between 1947 and 1957 they tripled their total research and development expenditures and increased the basic research portion by a factor of 4.5. In the same period the Navy doubled research and development expenditures but increased the basic research portion by a factor of only 1.5. This increase in basic research expenditures was essentially offset by reason of the fact that the total cost per scientist increased approximately 50 percent during this same period.

A group of industrial directors of research familiar with the problems of the Navy were unanimous in their judgment that the Navy should increase the percentage of its research and development budget devoted to basic research.

To take advantage of the experience gained by industry in establishing corporate research and development budgets, we sought the opinions of leading industrial directors of research on Navy participation in basic research. The thirty-three men approached for opinions administer almost one half of industry's basic research expenditures and are responsible for allocation of funds within their respective corporate research and development budgets. Sixteen of the thirty-three believed they had sufficient knowledge of the Navy and its missions to be willing to express a judgment. Given the task of constructing a research and development budget for the Navy considering its missions, size, technical complexity, strength of Soviet competition, and the severe consequences which would be faced for being second best in national defense at this stage in history, it was the judgment of the majority that the resulting budget should show basic research in the range of 15-20 percent of the total research and development effort. An aggressive approach to participation in basic research is demanded, since nowhere is success more important today than in military technological advance.

In general, the greater the technological strength of the competition and the less immediate the probability of conflict, the greater should be the emphasis on basic research. Thus, under such conditions, the nature of weapons which might be used against this nation, and the countermeasures which might be employed, become less predictable, forcing a broadening of the basic research effort. Conversely, basic research plans can be more specifically drawn if conflict appears imminent.

the greater the technological strength of the competition, the greater should be the emphasis on basic research

Although there is legitimate widespread concern about a national shortage of scientific manpower, the Navy should find this no immediate obstacle should it decide to increase its basic research effort.

With any substantial increase in Navy participation in basic research, the problem of availability of competent scientific manpower will arise. At this moment it appears from a study of meritorious proposals turned down, or discouraged prior to submission, that sufficient manpower

exists to expand the Departm[illegible] of Defense basic research effort in outside contracts by approximately 70 percent (omitting certain large capital equipment proposals). In addition, a rough approximation indicates an increase of about 10 percent is currently possible in the Navy in-house basic research effort. However, a serious manpower shortage may well develop in the near future as national research and development activities are currently expanding at the rate of 10 percent per year, whereas the number of scientists and engineers is increasing at the rate of 5 percent per year. At present approximately 25 percent of scientists and engineers are engaged in research and development activities, but only about 2 percent are engaged in basic research.

An expansion of the Navy basic research effort will place a premium on improved program planning and communications. The former might be achieved through greater use of scientists in a consulting capacity. The latter will require continuing study and emphasis since more than one half of the work performed will be outside of Navy laboratories and widely distributed geographically.

Because of the length of time required to evolve results, Federal budgeting for basic research presents special, and as yet not completely resolved, problems.

Budgeting for basic research is complicated by the necessity for planning on a long-term basis, while budgeting and operating on an annual basis. Planning basic research must take into account the time needed to form the research team, perform experiments and analyze and publish the results. The over-all time required for this process, as measured by the current average life of Office of Naval Research projects, is about 5 years.

Considerable progress in budgeting has been made through the availability of no-year money (available until expended) and advance financing of research projects. These tools are limited, however, by the amount of funds made available each year in the face of stiff competition offered by current fleet requirements particularly at times of expenditure curtailment or limitation. In order for the Navy to establish a more aggressive basic research program, methods must be found for budgeting and contracting on a basis which will tend to allow longer range planning and eliminate damaging annual variations. This is a problem of broad national interest, involving many agencies in addition to the Navy Department. The solution rests in large measure on bringing about a better understanding and appreciation of the role of basic research to provide the basis for coordinated budget planning by the Executive Branch and Congress.

It may be possible to develop a mathematical model of the relationship between segments of the research process that would aid in determining a proper level of Navy participation in basic research.

A program to develop a mathematical model of the relationship between the segments of the research process has shown enough promise to warrant consideration for further development. Results obtained by trying to fit a few actual case histories into the model as it now stands have been encouraging. However, more time is needed to substantiate the basic assumptions of the model, and the relation between what it predicts with respect to a proper level of basic research and what is observed in the real world.

Supplementary Observations

There exists within the Navy Department a general belief that the Office of Naval Research is the sole Navy office authorized to finance basic research. This misunderstanding stems largely from budget procedures, and has led to some confusion as to the extent of the Navy basic research effort. In addition, it has handicapped the administration of Navy laboratories in initiating basic research programs. Corrective steps and education are required.

Among Department of Defense laboratories, basic research contributions by the Navy laboratories are outstanding. This is especially true of the Naval Research Laboratory, which writes approximately 30 percent of all scientific papers originating in Department of Defense laboratories. The Naval Ordnance Laboratory, Naval Ordnance Test Station, Naval Electronics Laboratory, and others also make significant contributions. Knowledge generated in these basic research programs has contributed significantly to Navy effectiveness.

among Department of Defense laboratories, basic research contributions by the Navy laboratories are outstanding

This study is, so far as could be determined, the first of its type for the Government. In performing research on research, investigators are immediately confronted with the handicap of woefully inadequate data. With total research and development expenditures now amounting to approximately 6 percent of the Federal budget, more study of research is indicated. This is the path to improved national policies from which will emerge more effective utilization of our scientific resources. Some of the techniques developed or employed during the course of this study appear worthy of refinement and application by the Navy to such areas as:

a. *Research planning*

It should be possible to plan more effectively expenditures in basic research through detailed analysis of such factors as the so-called barrier problems within fields of interest to the Navy, and the relative world-

wide research activity within such fields through literature investigations, coupled with study and evaluation of scientific manpower. Machine techniques and mathematical models may become useful in this regard.

b. Intelligence

Analysis of world-wide basic research activities by advanced techniques should offer excellent opportunities for progress in the field of intelligence.

Introduction

Ever since World War II, research has been increasingly recognized as a vital factor in the national defense. This has been emphasized in numerous studies since that time. However, only by a properly balanced and administered program of basic research, applied research, and development can the rapid evolution of new weapons systems and techniques of warfare be reasonably assured. A major unsolved problem is the determination of what consitutes a properly balanced research program at any given time in history. This is a matter to which attention must be directed if the United States is to carry out effectively its policy to deter war and to repel and decisively counter any possible attack.

The Department of the Navy has been aware of the essential role of scientific research in military preparedness. This is evidenced by the formation of the Office of Naval Research in 1946 (Public Law 588) and the vigorous programs of research and development carried out by this Office and the Bureaus of the Navy Department and Marine Corps since that date. A mechanism for continuing review of the Navy program was also provided in Public Law 588 through the establishment of a Naval Research Advisory Committee, to be composed of persons pre-eminent in the fields of science and research. The purpose of this Committee is to consult with and advise the Secretary of the Navy, the Chief of Naval Operations, and the Chief of Naval Research on matters pertaining to research and development.

In reviewing the research program of the Department of the Navy in 1957, the Naval Research Advisory Committee addressed its attention in particular to the problem of over-all balance. It was agreed that the most perplexing problem in connection with achieving a balanced research program for the Navy was the establishment of an appropriate level of participation in basic research. In the furtherance of its missions, it is evident that the Navy must undertake basic research, but the proper

level of effort to be devoted to such work could not be clearly defined by the Committee despite extended discussions. At the same time, the importance of reaching sound decisions on this matter was recognized to be of great significance to the future capabilities and striking power of the Navy and Marine Corps.

As a result, the Navy Research Advisory Committee recommended that the Office of Naval Research enter into a contract with an outside agency to study this problem in detail. Accordingly on February 1, 1958, a contract was initiated with Arthur D. Little, Inc., having the following scope:

> Perform a study to determine a basis for decision as to the proper level of support of basic research by the Department of the Navy. Such a study is to be conducted through interviews, data collection, case histories, and other appropriate means.
>
> Prepare a report describing in detail the results of said study, and also prepare a monograph setting forth as clearly as possible the principal conclusions and recommendations resulting from the study.

Volume I of this report constitutes the monograph resulting from the study. Volume II, a detailed report with accompanying data and appendices, has been submitted separately to the Secretary of the Navy.

As far as could be determined, no formal study of this type has previously been published. The difficulty and complexity of the problem were apparent at the outset. Therefore, every attempt was made to take advantage of the suggestions, judgment, and experience of persons knowledgeable in the general field of research and its administration within Government, industry, university, and institute circles. The cooperation and response were excellent as the subject is one of great and increasing interest.

Navy Dependence on Technology— A Brief History

The First Congress authorized the Navy in 1789 to experiment on ships and guns. From the time of its formation in 1798 the Navy Department, in recognition of its dependence on technology, has been a leader among the armed services in conducting, financing, and encouraging research and development pertaining to its missions. This policy has had a profound influence on the continued gain in effectiveness of the Navy in all aspects of its operations.

While active research was going on in universities in Europe and the United States to increase fundamental knowledge, there was almost no organized research in industry in the United States prior to 1900. Its early activities thus placed the Navy in a position of leadership, and the technological advances of the Navy in its own mobility, firepower, communications, personnel operations, endurance, and supply had a great effect upon technical progress made in the civilian economy. The importance attributed by the Navy to its dependence on technological innovations is clearly evident from a chronological study made of its many contributions.* These range all the way from instigating the establishment of the National Academy of Sciences to the measurement of the velocity of light, the development of smokeless powder and the development of the aircraft carrier. The chronology includes such famous names as Fulton, Maury, Dahlgren, Davis, Michelson, Munro, Durand, Hunsaker, Taylor, and Sperry.

A New Era

Despite the great significance of early technological milestones in the development of a Navy second to none, they were abruptly paled into comparative insignificance by the advent of a new era. Simultaneously with the gathering of war clouds in Europe in 1939, there began a period

* See Volume II of this report for a Chronology of some 300 Navy historical technical milestones.

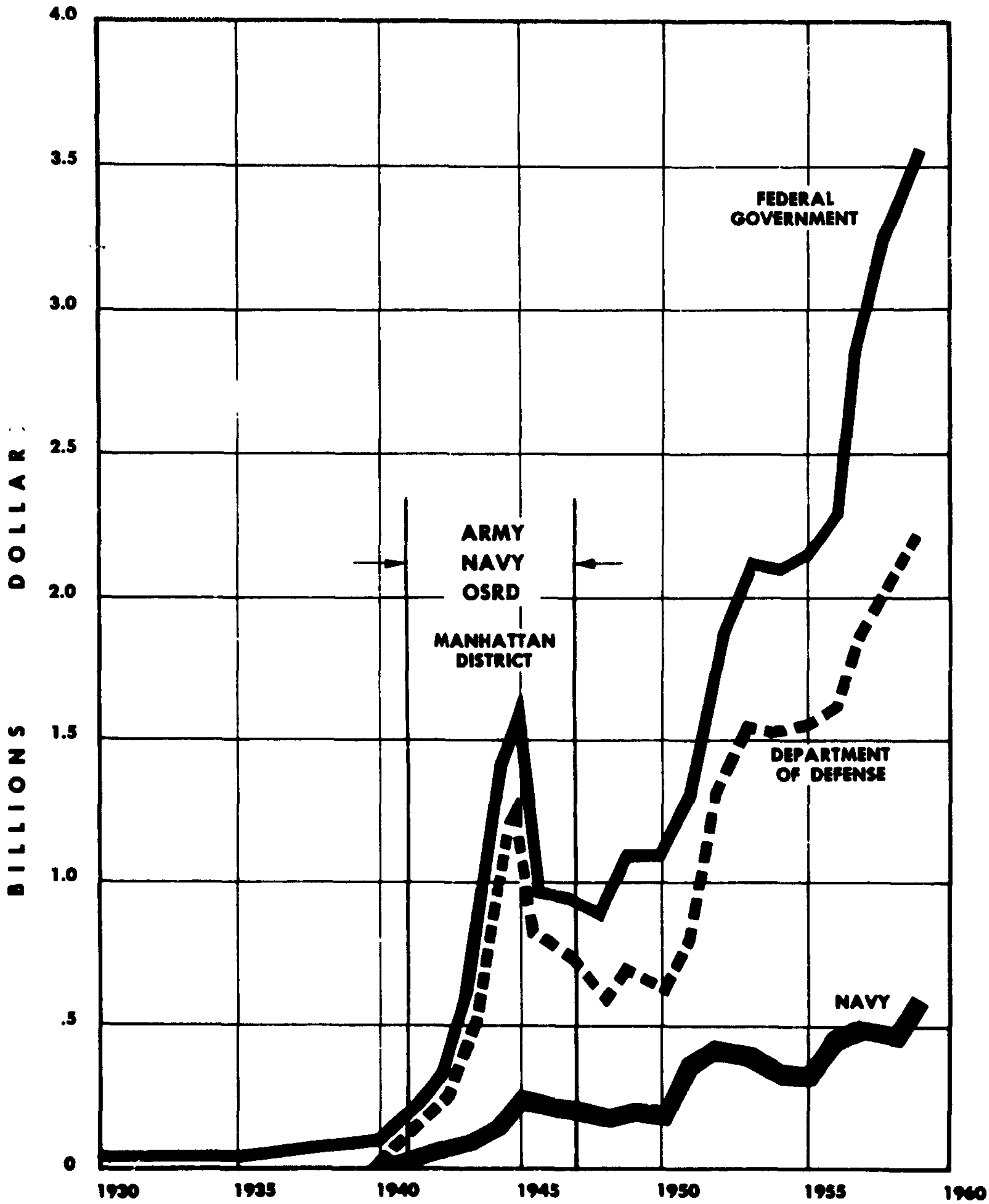
which will be known in history as the era in which science became a dominant factor in the determination of the military posture of the United States.

The birth of this stage of history was the direct result of actions taken by leading scientists. The two most important early steps involved direct contact with the White House in order to obtain the backing of the President. The first action was taken in the summer of 1939 by Albert Einstein, Enrico Fermi, Leo Szilard, and Eugene P. Wigner when they interested President Roosevelt, through Alexander Sachs, in the potential military importance of uranium. The President proceeded to appoint an Advisory Committee on Uranium under the chairmanship of Dr. Lyman J. Briggs, Director of the National Bureau of Standards. The Committee first met in October, 1939. At almost the same time Dr. Vannevar Bush, Chairman of the National Advisory Committee for Aeronautics, began to formulate plans for a National Defense Research Committee to:

> "coordinate, supervise and conduct scientific research on the problems underlying the development, production, and use of mechanisms and devices of warfare, except scientific research on flight"

Dr. Bush met with President Roosevelt early in June 1940, and an Executive Order establishing the new agency was signed June 27, 1940. Top scientists of the nation immediately volunteered their services and joined in organizing and directing the effort. The powers of Dr. Bush were extended June 28, 1941, by another Executive Order which created the Office of Scientific Research and Development and also placed under his direction research on military medicine. From a modest beginning in keeping with Government research expenditures of prior years, the NDRC grew in giant strides as the military contributions of science became obvious. Similarly the Advisory Committee on Uranium underwent major changes, the outgrowth of which was the rapidly expanding atomic bomb project of the Manhattan Engineer District.

The resounding impact on Federal research and development expenditures resulting from the aforementioned White House visits of 1939 and 1940 is shown by Figure 1. The era of scientific and technological impact on the national defense is clearly evident from the rapidly rising sums invested in this field. The many successes in increasing military effectiveness through such outstanding developments as radar, nuclear explosives, proximity fuzes, automatic fire control, rockets, guided missiles, jet aircraft, and numerous aspects of military medicine are well-known. Unlike the past, technological advances have become so rapid that many weapons systems are obsolescent by the time they reach the production stage. In order to preserve the peace, we find ourselves in an accelerating race of measures and counter measures, missiles and anti-missiles.



FEDERAL RESEARCH AND DEVELOPMENT EXPENDITURES

. . . . the impact of research for national defense

Figure 1

Rise of the Office of Naval Research

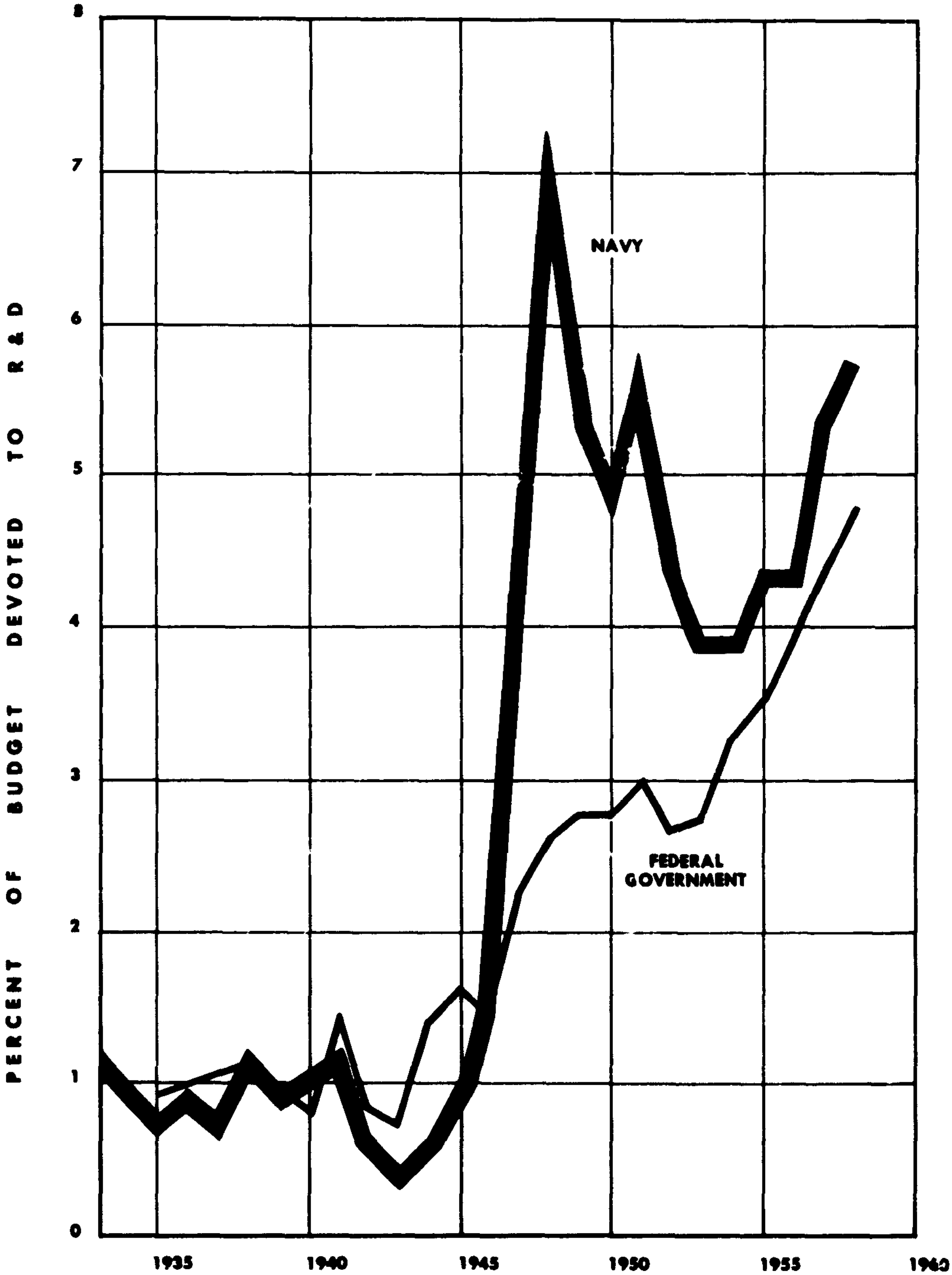
Throughout this era the Navy has continued its tradition of aggressive participation in research and development as shown by the growth in its expenditures (Figure 1). During World War II this growth was slowed by the absorption of technical manpower by the OSRD and the Manhattan Project. As the war neared its end, the Navy became alarmed at the possibility of a general exodus of scientists and engineers from research and development on subjects of interest to the Navy. A strong urge was quite naturally developing among them to drop everything of a military nature with the cessation of hostilities and return to their former peacetime pursuits. This possibility was disturbing, as the war years had taught top Navy personnel how essential it is to maintain close contact with the scientific world. Without such contact in a period of rapid technological change, they foresaw it would be impossible for the Navy to make the technical advances so necessary to the performance of its missions.

the war years taught top Navy personnel how essential it is to maintain close contact with the scientific world

Thus the Navy Department began early in 1944 to make new plans for the future. Conferences were held with top personnel in the OSRD and the armed services. Plans were initiated to establish a central office in the Navy Department which would foster research. It was as an outgrowth of these that the Office of Naval Research was formed in the Office of the Secretary of the Navy in 1946.

Great care was taken in establishing the policies of the Office of Naval Research with respect to the planning, negotiation, and administration of research contracts. The Navy then moved to allocate a much higher percentage of its budget to research and development. In this move the Navy took a substantial lead over the rest of the Federal Government (as shown in Figure 2).

In the planning of its early program, the Office of Naval Research had the assistance of many of the top scientists of the nation, most of whom were familiar with the long-range problems of the Navy because of their wartime experience. In general they urged that the Office of Naval Research place a large share of its budget in basic research. They pointed out that only a small percentage of the increasing expenditures shown in Figures 1 and 2 were devoted to basic research. They believed a change in this policy was essential to the long range military strength of the Navy as well as the nation. It was decided to rely on the judgment of these leaders in science. The Office of Naval Research placed as much as one third of its total funds in basic research and became a major national factor in this field. Many scientists credit this substantial post-war participation in basic research by the Navy, within its own laboratories and through outside contracts, for the current high stature of science in the United States. This pioneering effort made easier the entry of other



RESEARCH AND DEVELOPMENT PORTION OF FEDERAL BUDGET

. . . . the navy leads the trend

Figure 2

agencies such as the Army, Air Force, Atomic Energy Commission, National Science Foundation, and Department of Health, Education, and Welfare into basic research. In fact, the Office of Naval Research later transferred to these agencies contracts thought to be more closely related to their respective missions.

Basic Research—An Orientation

The research process is generally considered to be a continuous spectrum of activity composed of three major segments known as basic research, applied research, and development. The important roles of the applied research and development segments in evolving technological innovations are widely known to the general public. There are two main reasons for this. First, these are areas of research in which the Government and industry have long participated, so that the public has had time to learn of innumerable valuable contributions. And, second, when research has proceeded into these later stages, things begin to take shape. You can see and touch the new creations. They become newsworthy, and the public is kept well informed through aggressive press coverage.

the Federal Government did not begin to participate extensively in basic research until about thirteen years ago

But the basic research segment is different. The Federal Government did not begin to participate extensively either as a source of funds for, or as a performer of, basic research, until about thirteen years ago. Policies regarding administration and expenditures are still being formulated. The public has hardly had sufficient time to become familiar with this activity. In addition, many people believe they are incapable of understanding the implications of basic research, simply because it operates on the very frontiers of human knowledge. It is certainly true that knowledge of details in a particular field of science can be difficult, even for scientists working in related fields. However, such knowledge of details should not be confused with what is really significant for people to comprehend. It is far more important for the general public and Government officials, in this age of science, to acquire a broad understanding of the role of basic research in bringing about the advancement of technology. Such understanding is not difficult for the laymen to acquire, provided communications are clear.

The method to be used in this report to explain the nature and role of basic research is somewhat unique in that it will be partly diagrammatic. In this way it is hoped the primary objective of brevity can be coupled

with clarity. In Chapters III and IV six diagrams, which we will call schematic models, will be presented to illustrate the following points:

> The manner in which, following the discovery of one significant new fact, a whole new field of technology evolves with time through basic research.
>
> The dependence of technological innovations of importance to the Navy upon the acquisition of knowledge through basic research in many fields.
>
> The explosive expansion of a new field through the influence and work of a basic research scientist and those inspired by his guidance or leadership.
>
> The importance to the attainment of effective technological progress of establishing and maintaining close coupling between basic research and applied research and development.

What is Basic Research?

It is logical to begin an explanation of basic research by trying to define what is meant by this activity. In this study the following official Department of Defense was utilized:

> **"Basic research is that type of research which is directed toward increase of knowledge in science. It is research where the primary aim of the investigator is a fuller knowledge or understanding of the subject under study."**

As is so often the case in trying to convey the meaning of a general concept, this definition may leave something to be desired. Nevertheless, it is rather widely accepted as a broad definition of basic research. It stresses the significance of permitting the basic research scientist freedom to consider any and all avenues as he seeks to create new knowledge or understanding of the subject under investigation. Since he is exploring the unknown, such freedom to probe and to change course is essential. But effective basic research requires much more than the interrogation of nature through theoretical and experimental study to discover new facts. It also involves ordering these facts into a pattern and communicating them unambiguously to others. This communication is achieved largely through the medium of papers published in scientific journals. Such papers represent the output of basic research. In general this is the only thing you can see or touch, the only tangible evidence emerging from the basic research efforts of a man or a laboratory.

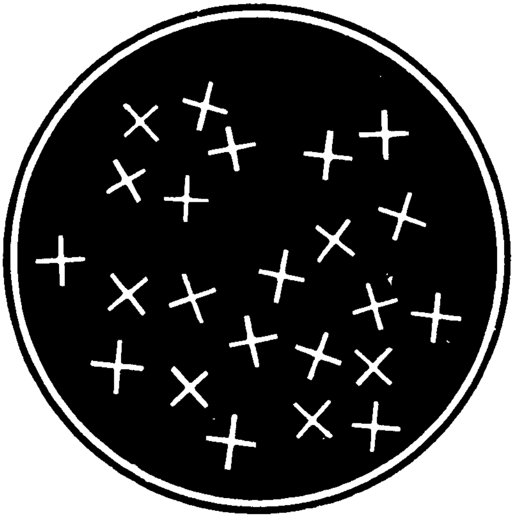
Since most of these papers are highly complex, difficult to read, and their significance usually grasped only by a few other scientists, they

seldom receive the plaudits of the popular press. Yet these very same papers represent the basic building blocks, the new scientific knowledge, from which spring advances in the national welfare, economy, and military strength. These advances generally require some years to develop. This is because opportunities presented by basic research must be followed by the equally important applied research, development, and production phases before full fruition is realized. By this time there is a tendency to forget the contributions of basic research which made the entire advance possible in the first place. Its significance, difficult to recognize when first recorded in scientific papers, is further dimmed by the passage of time. Even the men who carried out the basic research may well be making their contributions in another field in a different location, and be no longer interested in or connected with the advance.

progress in basic research takes place in spurts or jumps

Progress as basic research takes place in spurts or jumps, recently popularly tabbed as breakthroughs. These spurts are by their very nature unpredictable. While most of them spring from soil already well prepared by prior work, some are accidental. Following the spurt there is usually a period of decreasing rate of progress until another one occurs. The gross characteristic of a plot of some measure of efficiency as a function of time will present a series of steps, with each vertical rise larger than that preceding, while the successive horizontal time intervals become shorter. By way of example, the development of projectile weapons from arm power using rock, spear, or boomerang; through mechanical devices such as bow and arrow, catapult, arbalest, or crossbow; then chemically propelled projectiles from hand and shoulder weapons through long-range artillery; then bomb-carrying aircraft; now missiles; and presumably the manned-satellite show such a development for the plot of progress as a function of time.

Another way of thinking about each spurt or breakthrough is that it opens up a new field of research. This field consists of a large number of facts, connected by some relationship to each other, and all unknown before the field is open. This situation is represented schematically in Figure 3 (a). While any one of the facts (represented by x's)in this field could have been discovered, it is typical that until the spurt no one thought of looking for them or thought it worth the effort to look. But once the field opens up, people realize in a vague way that facts are there and basic research is performed to find them. Knowledge is pursued for the sake of knowledge or understanding, and thoughts of application are usually latent. This is the phase in which Faraday was working in the new field of electricity when, on being asked of what use his work was, he replied, "What is the use of babies? They grow up!"



a

b

A NEW FIELD IS DEFINED

DISCOVERED FACTS  UNDISCOVERED FACTS

Figure 3

After basic research has been carried on for a time, the situation develops to that shown in Figure 3 (b). Some of the facts have now been discovered (represented by circled x's). These facts now known suggest that certain specific applications might be possible, particularly if suspected but undiscovered facts exist nearby. This is the origin of applied research which is impossible without the basic research which precedes it. Because it has a definite application in mind, applied research tends to concentrate in limited areas, indicated by the small dotted circle. By concentrating its effort in this way, it is apt to proceed more rapidly within the chosen area. On the other hand, basic research, which tends to explore the entire field, is more likely to find the fact which suggests a new application, or to discover a theory which immediately orders all the facts in the field into a pattern which then makes apparent numerous applications. Over-all concepts of this nature often involve understanding and assembling facts from several other fields of science.

the origin of applied research

From this generalized concept of basic research it is evident that this activity presents some publicity problems as far as exciting public interest is concerned. How, then, can this rather bleak scene be injected with a bit of fire and life? How can the initial segment of the over-all research process, so essential to our nation, be brought into proper focus to achieve public understanding? This is best accomplished by spotlighting a few examples taken out of the recent past.

The Shock Wave—A Case History

Our first schematic model will deal with the shock wave*. Many people are becoming familiar with it for the first time through the disturbances caused as jet planes crack the sound barrier. It has been selected as a model partly because it has received little other publicity and certainly cannot be classified as a shop-worn example of the value of research.

The soil was prepared for the discovery of the shock wave by earlier basic research work on the theory of sound performed by scientists from several nations such as Newton, d'Alembert, LaPlace, Lagrange, and Poisson. This field became the subject of discussions between two English physicists, Challis and Stokes, who were puzzled by certain problems in the solution of a mathematical equation, developed in 1808 by Poisson, describing flow in a gas. In seeking a unique solution, it was Stokes who in 1848 proposed that "a surface of discontinuity is formed, across which

*** A shock wave is defined as a compression wave propagating relative to the fluid into which it advances and having reached an equilibrium state in which the steepening effects of inertia and the broadening effects of viscosity and heat conductivity are exactly counterbalanced, so that the wave is of constant shape.**

there is an abrupt change in velocity and density." In 1860 this subject received further attention from two mathematicians, Earnshaw of Great Britain and Riemann of Germany. Working independently, they contributed significantly to the concept of the shock wave, Riemann developing new abstruse mathematical theories in order to do so. Then came Rankine of Great Britain and Hugoniot of France, who placed the subject of shock waves on a firm theoretical basis by correcting certain assumptions of their predecessors.

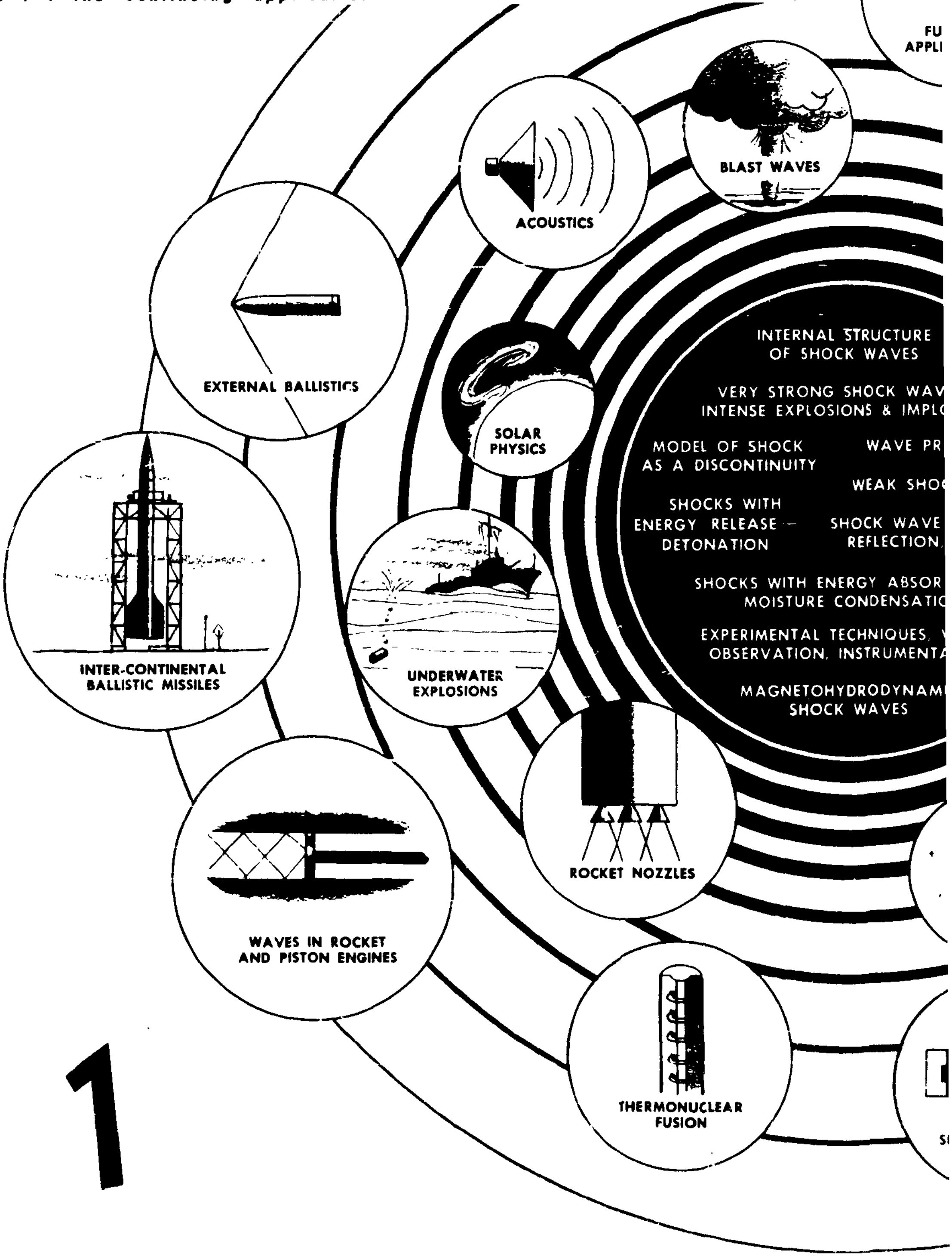
This brief story of the brilliant discovery of a new phenomenon is typical. At the time it must have been termed unexciting, vague, dreamy, and impractical. Certainly these six men could not predict the ultimate utility of their work, nor can we. But by 1959 the extensive and rapidly expanding basic research contributions, sparked by their initial efforts, have brought about over the years the development of things so diverse as to stagger the imagination. The multitude of present-day applications, many of importance to the Navy, are depicted in Figure 4. The ramifications of the work spread far and wide into such seemingly diverse areas as acoustics, explosives, jet engines, wind tunnels, rockets, satellites, underwater sound, solar physics, ballistic missiles, supersonic aircraft, and even thermonuclear devices.

But how did all of this come about? What happened between 1860 and today? The answer is that basic research performed throughout the intervening years has provided the additional facts required to permit subsequent applied research, development and production of numerous priceless technological innovations. As postulated in the general description of the research process, once the field of shock waves was opened up by Challis, Stokes, Earnshaw, Riemann, Rankine, and Hugoniot, then many basic research scientists explored for the facts. This involved scientists from many nations and many fields, principally physicists, mathematicians, chemists, and aerodynamicists. The work was performed in university, industry, and Government laboratories. Some of the key work was financed by the Navy Department. As facts emerged, scientific papers were written suggesting ideas to others, permitting cross-fertilization between sciences and the ordering of seemingly unrelated facts into theories. The field thus grows, first slowly, then at a rapidly accelerating rate. With all the interrelationships involved it is a most complex thing to picture. A schematic model of the history and development of shock wave theory is shown in Figure 5. Were all the side contributions of other sciences also shown, it would look even more like a large Chinese puzzle.

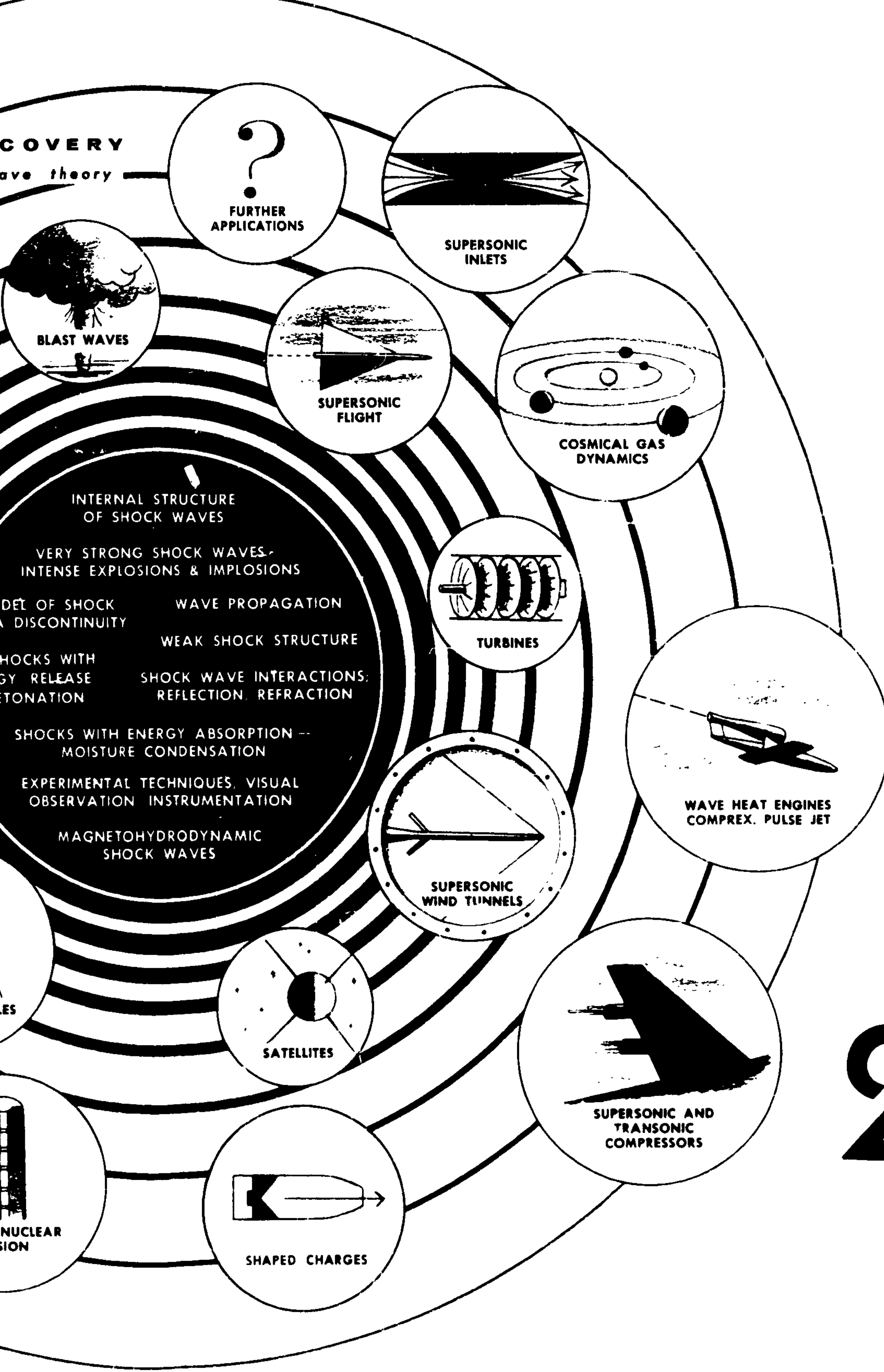
This complexity of growth is one of the interesting aspects of basic research. A seemingly remote fact may be the missing piece of a large picture puzzle, or its appearance in the scientific literature may trigger an important discovery. Once a fact is discovered and recorded, it is

THE VALUE OF A BASIC RESEARCH DISCOVERY

. . . . the continuing application of the field of shock wave theory



1


FURTHER APPLICATIONS
SUPERSONIC INLETS
BLAST WAVES
SUPERSONIC FLIGHT
COSMICAL GAS DYNAMICS
INTERNAL STRUCTURE OF SHOCK WAVES
VERY STRONG SHOCK WAVES - INTENSE EXPLOSIONS & IMPLOSIONS
WAVE PROPAGATION
WEAK SHOCK STRUCTURE
SHOCK WAVE INTERACTIONS: REFLECTION, REFRACTION
SHOCKS WITH ENERGY ABSORPTION -- MOISTURE CONDENSATION
EXPERIMENTAL TECHNIQUES, VISUAL OBSERVATION INSTRUMENTATION
MAGNETOHYDRODYNAMIC SHOCK WAVES
TURBINES
WAVE HEAT ENGINES COMPREX, PULSE JET
SUPERSONIC WIND TUNNELS
SATELLITES
SUPERSONIC AND TRANSONIC COMPRESSORS
SHAPED CHARGES

2

always there to use. Some are seldom used, others are used over and over again and the compound interest derived by man is almost beyond measure.

The question is often asked whether there are not cases where progress in a field such as this has been stalled because of lack of basic research. The answer is certainly in the affirmative. Yet, characteristically, it is difficult to foresee such void areas until after the fact. For example, one can state that progress in ballistic missile nose cones would have been more rapid if we had performed at an earlier date more studies of heat transfer phenomena in shock tubes. But the pioneering work in shock tubes was carried out at a time when the practicality of long-range ballistic missiles seemed quite remote. Thus one is equally free to condemn our lack of foresight, or to commend the Navy for its early participation in shock tube research (see Figure 5, 1950) which ultimately provided, albeit later than desired as we look back on it, the vitally important heat transfer information. What this really means is that a forward spurt by basic research in one field often exposes the need for basic research in the same and other fields. If we can be sufficiently foresighted to predict such things, much time will be gained. Study of the planning of research by a more systematic and thorough analysis of world-wide literature, scientific manpower evaluation, and by seeking out the so-called barrier problems in each field might well improve our abilities in this vital area. At a minimum, participation in basic research provides the Navy, as indicated by this example, the means to move rapidly at the moment the impact of a discovery in one field brings demand for knowledge from another field.

the performers of basic research

It is appropriate at this time to consider briefly the people who performed this work. They are anything but a collection of queer, long-haired, white-coated recluses, as science fiction would have us believe. Some are creative, some inductive, some cumulative and descriptive, some meticulous, and others routinely industrious — all types being essential to the growth of the field. Some remained in science all their lives, many as outstanding university professors. G. G. Stokes was himself the Lucasian Professor of Mathematics at the University of Cambridge, the chair once occupied by the illustrious Sir Isaac Newton. Some have become household words, such as Mach whose name is daily used in describing the speed of flying aircraft, missiles, and space ships. Many have remained essentially unknown. Others have become great public figures like Nobel Prize winning Lord Rayleigh of England and Dr. John von Neumann, the late member of the U. S. Atomic Energy Commission. Some like T. Von Karmann, G. B. Kistiakowsky, E. B. Wilson, and H. A. Bethe have devoted considerable time in recent years working at policy-making levels to enhance the technological strength of the United States. Others like G. I. Taylor, a brilliant and prolific contributor to science, found time to make contributions in completely different areas, such as the profitable invention of an anchor. Thus, the cross section of

Figure 5

The Shock Wave — A Schematic Model of the Development of a Field of Technology

The purpose of this schematic model is to show at a glance the typical manner in which, following its initial discovery, a whole field of technology evolves with time through basic research. Recorded on the chart are the major basic research contributions in the field of shock waves since its discovery in 1848.

Several general impressions are to be gained. Foremost is that the process of growth of a field of technology is complex. It has required the efforts over the years of numerous scientists from different nations. As shown by the mixture of colors and symbols, cross fertilization between fields of science is a necessary part of the process. Work builds on the achievements of the past, and accelerates with time as is evident from the growing density of work in recent years. Applications of vital importance to our national and military posture develop along the way with increasing frequency as already depicted in Figure 4.

The model is arbitrarily divided into five sections made up of research on shock tubes, explosions and detonations, magnetohydrodynamics, supersonic aerodynamics, and a central column devoted to continuing research in shock wave theory. There is considerable interaction between the sections, but cross-connecting lines have been omitted for purposes of simplification of the figure.

As shown at the top of the model, it all began in 1848 with the brilliant observations of two physicists, Stokes and Challis of England. Subsequent work by other physicists and mathematicians such as Earnshaw, Riemann, Rankine, and Hugoniot placed shock wave theory on a firm basis about 1890. From that time on, basic research in physics, chemistry, mathematics, and aerodynamics expanded the field at an accelerating pace.

Starting at the left, note that shock tube research was originated by Vielle in 1899. It was not until the work of Bleakney and co-workers in 1949 that the United States contributed significantly to shock tube research. The Navy actively participated in backing the work of Hertzberg and Kantrowitz at Cornell. This later led to studies of hypersonic flight at Mach 25, and is continuing to make important contributions to the intercontinental ballistic missile and space flight programs.

The first work on detonations and explosions around 1900 by Chapman and Jouguet concerned itself with combustion studies and propagation of flames. Much of the important work by people like Friedrichs, Kistiakowsky, and Von Neumann was performed under OSRD, Army or Navy contracts. Theories developed were utilized in the design of the first atomic bomb.

Magnetohydrodynamics is a relatively new field, having been opened up by Alfven of Sweden in 1942. Work is now rapidly expanding through the efforts of such men as Teller, Fowler, Spitzer, and many co-workers because of the interest in connection with nuclear fusion, solar corona, and various space age problems.

The development of supersonic aircraft is one of the most striking examples of the application of shock wave theory. This is an area of work in which basic research contributions of note have been made, beginning about 1900, by such men as Prandtl, Ackeret, Von Karmann, Taylor, Lighthill, and Lin. Other consequences of this work have been the development of nozzles, diffusers, and compressors for jet engines and rockets.

The central core research on better basic understanding of shock wave theory and structure has continued ever since 1848. Of late it has received the attention of an illustrious group of scientists such as Bethe, Courant, Chandrasckhar, Friedrichs, Taub, Weyl, and Von Neumann. What will next evolve in the way of startling new applications from the intriguing field of shock wave research is beyond our ability to predict. Meanwhile, active work in this field continues, with Government participation coming largely under the auspices of the Department of Defense, Atomic Energy Commission, and National Aeronautics and Space Administration.

1848

1860

1864

1870

1876

1880

1887

1889

1893

1899

1

P. VIELLE—1899
Origin of shock tube:
study of detonations

J. CHALLIS—1848
Velocity of sound

G. G. STOKES—1848
Breakdown of simple wave in sound propagation

S. EARNSHAW—1860
Mathematical theory of sound; shock problems in compressible fluid flow

BERNHARD RIEMANN—1860
Propagation of waves of finite amplitude in compressible two-dimensional medium

A. TOEPLER—1864
Origin of Schlieren method for visual observation of flow

W. J. M. RANKINE—1870
Thermodynamics of waves of finite longitudinal disturbance; correct shock conditions

E. MACH & J. WOSYKA—1876
First consideration of interaction of shock waves

V. DVORAK—1880
Shadowgraph method of flow observation

E. MACH & L. SACHER—1887
Photographic observation of supersonic projectile

H. HUGONIOT—1889
Correct shock conditions; Rankine-Hugoniot shock conditions

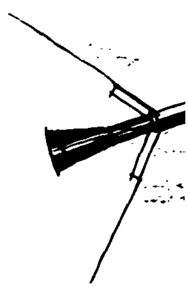
E. MACH & L. MACH
1889
Interferometer method for observation of supersonic projectiles

H. B. DIXON—1893
Rate of expansion in gases

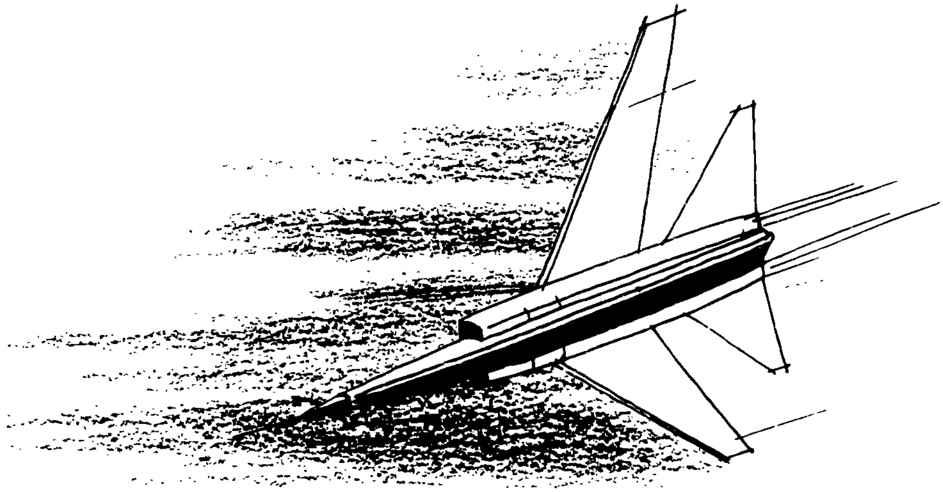
CHAPMAN—1899
Strong shock waves; detonations

THE EVOL

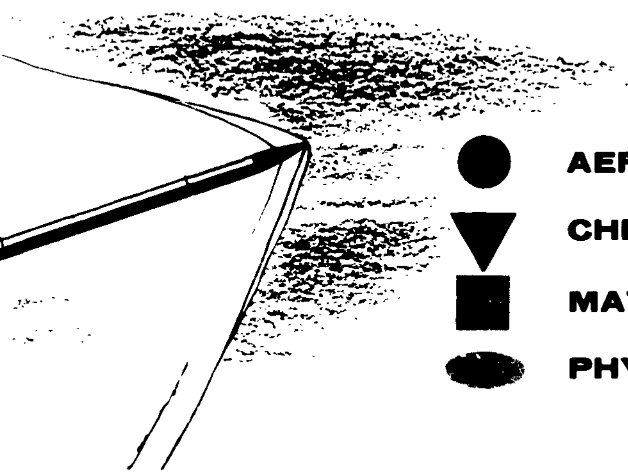
TH



2

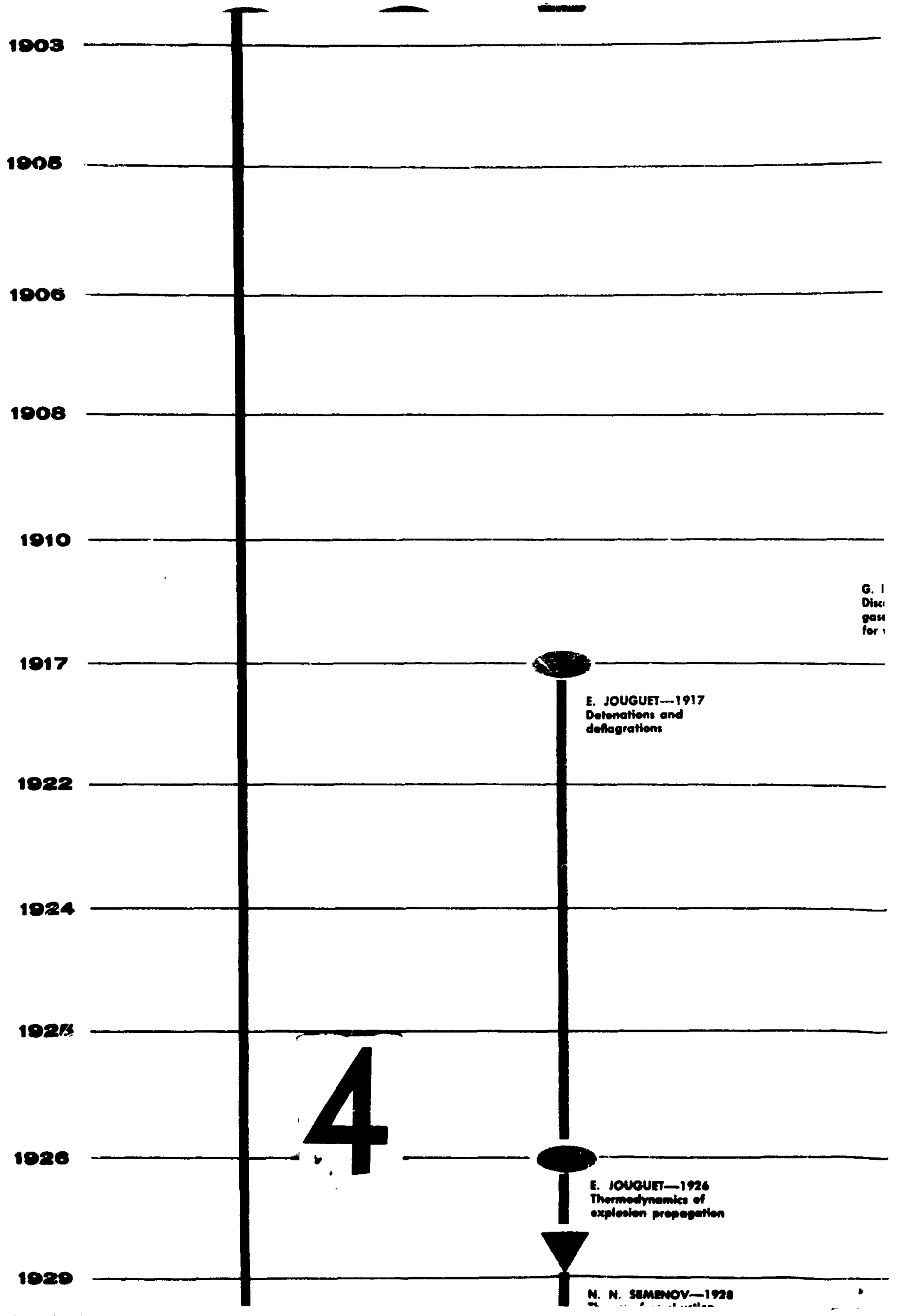


UTION OF SHOCK WAVE THEORY ROUGH BASIC RESEARCH



- AERODYNAMICS
- CHEMISTRY
- MATHEMATICS
- PHYSICS

3



1903
1905
1906
1908
1910
1917
E. JOUGUET—1917
Detonations and
deflagrations
1922
1924
1925
4
1926
E. JOUGUET—1926
Thermodynamics of
explosion propagation
1929
N. N. SEMENOV—1928

J. HADAMARD—1903
Conditions for vortex-free flows through shock waves

E. JOUGUET—1905
Detonation shocks; Chapman-Jouguet processes

G. I. TAYLOR—1910
Discontinuous motion in gases. Classical solution for weak shock structure

LORD RAYLEIGH—1910
Justification of Rankine-Hugeniot conditions on basis of viscosity

R. BECKER—1922
Thickness of a shock zone; Navier-Stokes shock structure

A. E. LOVE & F. B. PIDDUCK—
Interaction phenomena—Lagrange's ballistic problem

1903

L. PRANDTL—1904
Particle speeds before
and after shock

1905

1906

L. PRANDTL—1906
Thickness of the
shock zone

1908

T. MEYER—1908
Shock conditions for
steady plane flow

1910

1917

1922

1924

A. STODOLA—1924
Condensation shocks
in nozzles

1925

6

J. ACKERET—1925
Approximation methods for
shock conditions on
supersonic airfoils

1926

T. E. STANTON—1926
Flow of gases at
high speeds

1929

Theory of combustion
processes

1930

LEWIS & FRIAUF—1930
Experimental tests of
Chapman-Jouguet Theory

1931

W. PAYMAN—1931
Explosion wave and shock
wave experiments

1932

1933

1934

1935

1936

1937

W PAYMAN & W. C. F. SHEPHERD
—1937
Shock tube in ignition
studies

1939

1940

Y. B. ZELDOVICH—1940
Propagation of detonation
waves

8

A. BUSEMANN—1929
Representation of shock in hodograph plane; shock polarization

PRANDTL & BUSEMANN—1929
Fixed characteristics and their relation to Mach lines

1930

PRANDTL & BUSEMANN—1930
First supersonic wind tunnel of free-jet type

PRANDTL & GLAUERT—1930
Lift corrections at high speeds

1931

P. S. EPSTEIN—1931
Air resistance of projectiles

1932

TH. von KARMAN & N. B. MOORE—1932—Drag on slender bodies at supersonic velocities

G. I. TAYLOR—1932
Application of Ackeret's theory to supersonic airfoils

1933

G. I. TAYLOR & J. W. MACOLL—1933
Air pressure on cone moving at high speeds

1934

F. I. FRANKL—1934
Supersonic flows with axial symmetry

1935

A. BUSEMANN—1935
Aerodynamic properties of swept-back wings

1936

C. FERRARI—1936
Conical flow theory

1937

9

L. CROCCO—1937
Differential equations for non-isentropic flow

1939

A. FERRI—1939
Experiments in Guidonia supersonic wind tunnel

L. PRANDTL—1939
Sound propagation for fast moving bodies

1940

GUDERLEY & TOLLMEIN—1940
Shape of shock waves in two-dimensional flow

1941

W. PAYMAN & W. C. F. SHEPHERD 1941 — Disturbance produced by bursting diaphragms with compressed air

G. I. TAYLOR—1941 Detonation waves

KISTIAKOWSKY & WILSON—1941 Theoretical calculations on explosion produced shocks

1942

G. GUDERLEY 1942 — Implosion shock waves

J. von NEUMANN—1942 Theory of detonation waves

1943

W. DÖRING—1943 Relative velocity between shock wave and combustion

S. CHANDRASE-KHAR—1943 Conditions for existence of three shock waves

A. WEISE—19 Theory of inte and reflection

1944

G. I. TAYLOR—1944 Propagation and decay of blast waves

POLACHEK & —1944 Reflection of s ideal gases

1945

1946

PAYMAN & SHEPHERD—1946 Shock tube for high-speed chemical reactions

K. O. FRIEDRICHS—1946 Mathematical Theory of Detonations

1947

BRINKLEY & KIRKWOOD—1947 Propagation of strong shock waves

1948

10

1949

BLEAKNEY, WEIMER & FLETCHER —1949 Pioneer shock tube research in U. S.

GEIGER, MAUTZ & HOLLYER —1949 Use of shock tube as a short duration wind tunnel

1950

HERTZBERG & KANTROWITZ —1950 Extension of conventional shock tube to high temperature research

G. I. TAYLOR—1950 Very strong shock waves

S. TRAVER Shock wav

1951

A. HERTZBERG—1951 Modification of shock tube for hypersonic flows

CARRINGTON & DAVIDSON—1951 Dissociation of N_2O_4 by shock wave

PERRY & KANTROWITZ—1951 Imploding spherical waves

H. M. MOTT 1951 Structure of waves

1941

H. BETHE & E. TELLER—1941
Thermal equilibrium in shock waves

NEUMANN—1942
of detonation waves

H. BETHE—194.
Shocks in fluids with an arbitrary equation of state

H. ALFVEN—1942
Discovery of magneto-hydrodynamic waves

IANDRASE-
—1943
itions for
ence of three
waves

A. WEISE—1943
Theory of interaction and reflection

S. CHANDRASEKHAR—1943
Decay and interactions of shock waves

J. von NEUMANN—1943
Theory of shock waves; numerical methods

COURANT & FRIEDRICHS—1943
Interaction of shock and rarefaction waves; extension of Riemann's problem

POLACHEK & SEEGER—1944
Reflection of shocks in ideal gases

L. H. THOMAS—1944
Refined treatment of Becker's theory of shock front

H. WEYL—1944
Shock waves in arbitrary fluids

L. LANDAU—1944
Internal stability of gasdynamic discontinuities

M
Co
le
su

V. BARGMANN—1945
Reflection of shocks

47

A. H. TAUB—1947
Refraction of plane shock waves

C. S. WANG-CHANG—1948
Theory of thickness of weak shock waves

COURANT & FRIEDRICHS—1948
Interaction of shock and rarefaction waves

A. W. TAUB—1948
Relativistic Rankine-Hugoniot equations

J. HOUTGAST—1948
Shock waves in the solar atmosphere

BLEAKNEY & TAUB—1949
Interaction of shock waves

POLACHEK & SEEGER—1949
Interaction of shock waves in gases

11

S. TRAVERS—1950
Shock wave structure

J. M. BURGERS—1950
Weak shock structure

J. von NEUMANN & R. D. RICHTMEYER—1950
Numerical calculations of hydrodynamic shocks

F. de HOFFMAN & E. TELLER—1950
Magnetohydrodynamic shocks

WITZ—1951
g spherical

H. M. MOTT—SMITH 1951
Structure of strong shock waves

C. TRUESDELL 1951
Continuum theory approach to shock wave structure

COUSINS & WARE 1951 — Gaseous magnetohydrodynamic experiments; pinched electrical discharge

CARRUS, FOX, HAAS, & KOPAL —1951
Shock waves in a stellar model

V. L. GINZBERG—1951
Magnetohydrodynamic waves in gases

Z. KOPAL & C. C. LIN—1951
Propagation of spherical shock waves in stellar interiors

1941

TH. von KARMAN—1941
Compressibility effects
in aerodynamics

1942

...EN—1942
...y of magneto-
...namic waves

R. HERMANN—1942
Condensation shocks

F. SCHULTZ-GRUNOW—1942
Unsteady nozzle flow

1943

A. H. SHAPIRO—1943
Nozzles for supersonic flow
without shock fronts

1944

M. J. LIGHTHILL—1944
Conditions behind
leading edge of
supersonic airfoil

H. S. TSIEN—1944
Limiting line
in mixed flow

K. OSWATITSCH
1944
Buzz experiments;
spike-inlet diffuser

H. W. EMMONS—1944
Relaxation methods for
theoretical solution
to shock flows

1945

KANTROWITZ & DONALDSON
—1945
Limitations on convergent-
divergent diffusers

12

1946

R. T. JONES—1946
Oblique airfoils at
supersonic speeds

R. SAUER—1946
Plane gas waves with
compression shocks

ACKERET, FELDMAN, ROTT &
LIEPMANN—1946
Shock wave-boundary layer
interactions

1947

TH. von KARMAN—1947
Transonic similarity law

A. KANTROWITZ
1947
Normal shock waves
in channel flows

E. V. LAITONE
1947
Two-dimensional
oblique shock flow

F. I. FRANKL—1947
Shock waves in subsonic
flow with local
supersonic velocities

1948

...GAST—1948
...aves in the
...mosphere

MUNK & CROWN—1948
The head shock wave

D. C. PACK—1948
Shock waves in
supersonic jets

C. C. LIN, RUBINOV &
T. Y. THOMAS 1947-1949 —
Relations between head shock
waves and streamlines

1949

TSIEN & BARON—1949
Oblique shocks in
transonic flow

W. E. MOECKEL
1949
Form and location
of detached shock
waves

M. J. LIGHTHILL—1949
Shock strength in
supersonic conical fields

1950

...OFFMAN
...LLER—1950
...ohydrodynamic

R. von MISES—1950
Thickness of a
steady shock wave

T. Y. THOMAS—1950
Stability of
shock waves

MOORE & TAN—1950
Second approximations to
supersonic conical flows

1951

...PAL &
...IN—1951
...ation of
...al shock
... in stellar
...rs

FERRI & NUCCI—1951
Origin of instability of
inlet diffuser—single
shock conical diffuser

EVVARD & BLAKEY
1951
Perforated inlet
supersonic diffusers

NEUMANN &
LUSTWERK—1951
High efficiency
supersonic diffuser

L. MEYERHOFF—1951
Extension of theory
of one-dimensional
shock wave structure

PERRY & KANTROWITZ
1951
Production and stability
of converging shock
waves

1949

BLEAKNEY, WEIMER & FLETCHER —1949
Pioneer shock tube research in U. S.

GEIGER, MAUTZ & HOLLYER —1949
Use of shock tube as a short duration wind tunnel

1950

HERTZBERG & KANTROWITZ —1950
Extension of conventional shock tube to high temperature research

G. I. TAYLOR—1950
Very strong shock waves

S. TRAVERS—1
Shock wave st

1951

A. HERTZBERG—1951
Modification of shock tube for hypersonic flows

CARRINGTON & DAVIDSON—1951
Dissociation of N_2O_4 by shock wave

PERRY & KANTROWITZ—1951
Imploding spherical waves

H. M. MOTT—S
1951
Structure of stro
waves

1952

E. L. RESLER & OTHERS—1952
Production of high-temperature gases in shock tube

LaPORTE & OTHERS 1952 —
Luminous effects in shock tube spectral radiation of gases

1953

CARRINGTON & DAVIDSON—1953
Shock waves in chemical kinetics

GLASS & PATTERSON 1953—Theoretical and experimental shock tube studies

1954

S. C. LIN—1954
Cylindrical shock waves from instantaneous energy release

E. F. GREENE—1954
Chemical reactions in strong shock waves

SCHREFFLER & CHRISTIAN —1954
Boundary disturbance in high explosive shock tubes

1955

H. L. BRODE—1955
Numerical solution of spherical blast waves

1956

M. A. COOK & OTHERS—1956
Wave fronts in ideal and non-ideal detonation

1957

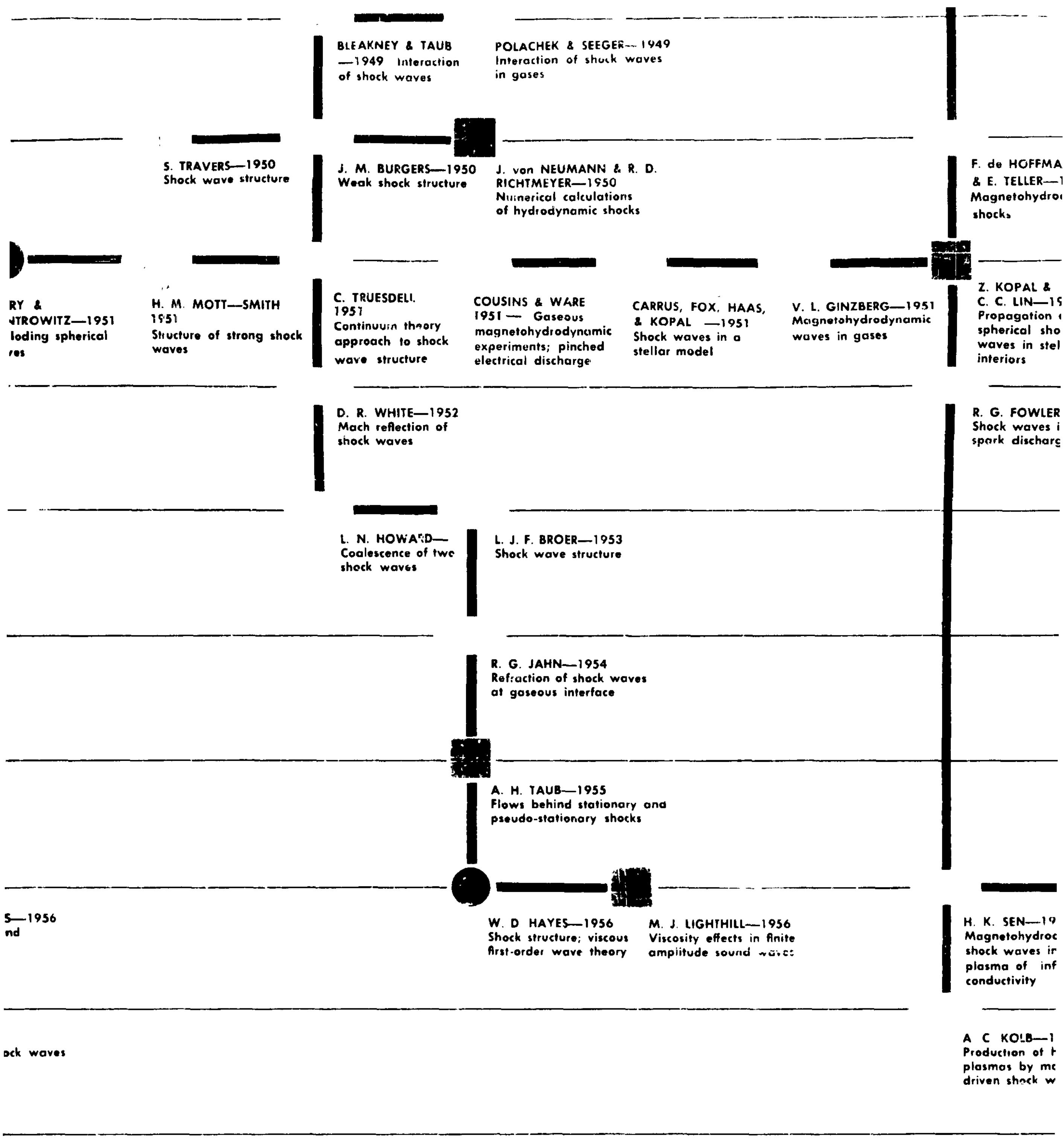
LAMB & LIN—1957
Electrical conductivity of thermally ionized air in a shock tube

D. F. HORNIG—1957
Energy exchange in shock waves

1958

SUMMERFIELD & McALEVY—1958
Shock tube as a tool for solid propellant research

SHOCK TUBE **DETONATIONS**



GENERAL RESEARCH MAGNETOHYDRODY

SHOCK WAVE RESEARCH

Figure 5

1949

TSIEN & BARON—1949
Oblique shocks in transonic flow

W. E. MOECKEL
1949
Form and location of detached shock waves

M. J. LIGHTHILL—1949
Shock strength in supersonic conical fields

1950

FFMAN
ER—1950
ydrodynamic

R. von MISES—1950
Thickness of a steady shock wave

T. Y. THOMAS—1950
Stability of shock waves

MOORE & TAN—1950
Second approximations to supersonic conical flows

1951

AL &
—1951
tion of
shock
stellar

FERRI & NUCCI—1951
Origin of instability of inlet diffuser—single shock conical diffuser

EVVARD & BLAKEY
1951
Perforated inlet supersonic diffusers

NEUMANN & LUSTWERK—1951
High efficiency supersonic diffuser

L. MEYERHOFF—1951
Extension of theory of one-dimensional shock wave structure

PERRY & KANTROWITZ
1951
Production and stability of converging shock waves

1952

WLER & OTHERS—1952
aves in low pressure
scharges

TING & LUDLOFF—1952
Aerodynamics of blasts

S. I. PAI—1952
Flow behind attached curved shock

1953

L. LEES—1953
Hypersonic flow—shock wave boundary layer interaction

1954

F. H. CLAUSER—1954
Ramjet diffusers at supersonic speeds

1955

1956

N—1956
hydrodynamic
aves in a
of infinite
vity

I KURCHATOV—1956
Production of high energy particles in intense pinched sparks

L. LEES—1956
Hypersonic flow—leading edge shock wave influence on luminar boundary layer

M. J. LIGHTHILL—1956
Viscosity effects in sound waves of finite amplitude

1957

OLB—1957
on of high energy
by magnetically
hock waves

1958

DYNAMICS

AERODYNAMICS

people contributing to a field of basic research is one of remarkably diverse talents. But all have the common characteristic of insatiable desire to know and understand the universe in which we live.

The people who perform basic research have at least one other common characteristic — they are exceedingly rare in number. Almost all of them have doctorate degrees. And of the 2% of college graduates who obtain a doctor's degree in science, only about one in five combines the creative skill and the motivation in our present American society to remain in basic research. Finally, about one half of these have outstanding talents for this field as indicated by the fact that they produce some 80% of the resulting scientific papers. The United States today has a total of only about 27,000 basic research scientists, of whom about 15,000 are particularly active. Many wise people sincerely believe their contributions to our welfare are all out of proportion to their number. It is for this reason that there is concern over establishing policies to permit fuller and more effective utilization of those scientists now existing, and concern over increasing the number now being trained.

the United States today has a total of only about 27,000 basic research scientists

Strength in Science Indicated by Nobel Prizes

As a last point in connection with orientation of the reader with respect to basic research, the matter of recognition is worthy of mention. There are few public honors accorded basic research scientists in the United States. This is one reason the public lacks understanding of the importance of basic research. The highest international honor in basic research is the Nobel Prize, first awarded in 1901. While there are numerous causes and effects influencing the rise and fall of the strength of a nation, it is most interesting to note the distribution of Nobel Prizes in science by nation since 1901, as shown in Figure 6. Study of this figure provides one more indication why participation in, and effective utilization of, basic research is properly a question of grave interest to the Government of the United States.

1

NOBEL PRIZES IN SCIENCE

. . . an international measure of achievement in basic research

NATION	1901 1910	1911 1920	1921 1930	1931 1940	1941 1950	1951 1958
	P	E	R	C	E	N T
● UNITED STATES	4	6	9	24	37	47
● UNITED KINGDOM	17	16	23	19	21	20
● GERMANY	33	40	32	24	16	7
● FRANCE	21	16	9	9	0	0
● RUSSIA	4	0	0	0	0	13
OTHER NATIONS	21	22	27	24	26	13

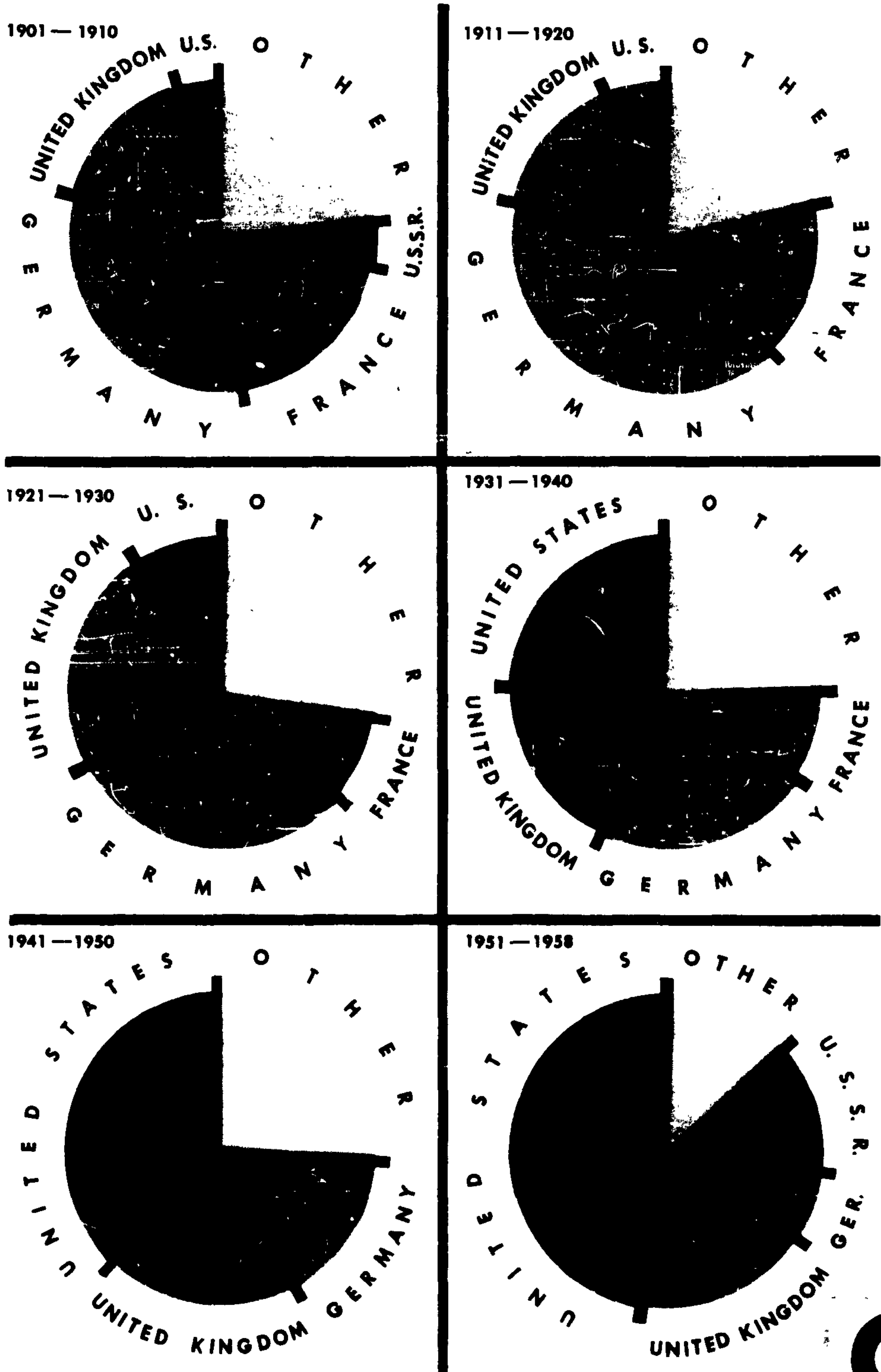

1901 — 1910
UNITED KINGDOM
U.S.
OTHER
U.S.S.R.
FRANCE
GERMANY
1911 — 1920
UNITED KINGDOM
U. S.
OTHER
FRANCE
GERMANY
1921 — 1930
UNITED KINGDOM
U. S.
OTHER
FRANCE
GERMANY
1931 — 1940
UNITED STATES
OTHER
FRANCE
GERMANY
UNITED KINGDOM
1941 — 1950
UNITED STATES
OTHER
GERMANY
UNITED KINGDOM
1951 — 1958
UNITED STATES
OTHER
U.S.S.R.
GER.
UNITED KINGDOM

Figure 6

2

The Relation of Basic Research to the Missions of the Navy

The purpose of this section is to inquire into various aspects of the relation of basic research to the furtherance of the missions of the Navy. It is necessary to understand this subject before conclusions can be drawn regarding Navy participation in basic research. As will be seen, perhaps the key consideration in this period of accelerating technological advance is that of need for rapid and effective communication by the Navy with the frontiers of science.

The National Science Foundation, formed in 1950, has as two of its major purposes the promotion of basic research and education in the sciences. Nevertheless, continuing participation in basic research by the Navy was contemplated. This is evident in Executive Order 10521, on the Administration of Scientific Research by Agencies of the Federal Government, issued March 17, 1954, by President Eisenhower. It includes the following:

> **"Section 4. As now or hereafter authorized or permitted by law, the National Science Foundation shall be increasingly responsible for providing support by the Federal Government for general-purpose basic research through contracts and grants. The conduct and support by other Federal agencies of basic research in areas which are closely related to their missions is recognized as important and desirable, especially in response to current national needs, and shall continue."**

Thus, in order to evolve a basis for decision as to the proper level of participation in basic research by the Navy Department, it is essential that the missions of the Navy be clearly understood.

The missions of the Navy as now officially decreed are as follows:

Seek out and destroy enemy naval forces and suppress enemy sea commerce.

Gain and maintain general sea supremacy.

Control vital sea areas and protect vital sea lines of communication.

Protect shipping.

Establish and maintain local superiority (including air) in an area of naval operations.

Seize and defend advanced naval bases.

As pertaining to these missions, the Navy is charged with a number of functions including:

> "Conducting research and development, including the development of specialized weapons and equipment."

It has been shown in Chapter I that throughout its entire history, Navy effectiveness has been heavily dependent upon the advance of technology. There is no argument over the fact that the Navy Department must have the right to foster a vigorous program of research and development if it is to carry out its assigned missions. The point in question is the extent to which the Navy must participate in basic research in order to maximize the effectiveness of its research and development in the furtherance of its missions. Opinions on this subject vary considerably. The explanation lies, in part, upon a lack of understanding of the role of basic research in bringing about technological innovations of importance to the Navy. As a first step, therefore, it is desirable to expand upon this subject.

Perhaps the best method of determining the importance of basic research to the development of equipment and components of great value to the Navy is to study some actual examples. This can be done by selecting technological innovations and then examining in some detail the manner in which they came into being. For this report we have selected radar as an example of equipment and the transistor as an example of a component. Both are of proven military value. Radar detection of German aircraft has been widely credited with saving England in World War II, and it also revolutionized naval warfare. The tiny transistor is currently revolutionizing the miniaturization of various missile guidance and computer devices.

Radar — A Case History

in research the only security is speed

History usually reveals that most great technological advances were the object of simultaneous investigations in a number of countries. Radar is a typical example in that we now recognize important work was proceeding in parallel in England, Germany, and the United States prior to World War II. It is positive proof of the fact that in research the only security is speed. Research in the United States leading ultimately to the development of radar, which revolutionized the means of detection of ships and aircraft, was begun at the Naval Research Laboratory in 1922.

On June 20, 1922, Guglielmo Marconi, the celebrated father of radio, in a speech in New York after accepting the Medal of Honor of the Institute of Radio Engineers stated:

> "As was first shown by Hertz, electric waves can be completely reflected by conducting bodies. In some of my tests I have noticed the effects of reflection and deflection of these waves by metallic objects miles away.
>
> "It seems to me that it should be possible to design apparatus by means of which a ship could radiate or project a divergent beam of these rays in any desired direction, which rays, if coming across a metallic object, such as another steamer or ship, would be reflected back to a receiver screened from the local transmitter on the sending ship, and thereby immediately reveal the presence and bearing of the other ship in fog or thick weather."

Shortly thereafter, while performing basic research on radio wave propagation, A. H. Taylor and L. C. Young of the Naval Research Laboratory obtained experimental confirmation of these speculations. Detection was made of a ship moving down the Potomac River. On September 27, 1922, a memorandum pointing out the possible Navy utility of such a detection system was transmitted to the Bureau of Engineering. The practicality of the idea could not be estimated at that time, and approval of a request to continue the work was denied. Interest in radio detection was renewed in 1930 at the Naval Research Laboratory when L. A. Hyland, during the course of experiments in radio direction finding, noted radio waves were reflected from aircraft which accidentally came within range. Again Taylor, Hyland, and Young were unsuccessful in obtaining funds to carry out a program of research in the field, as opinions of the practical importance of the work varied among both scientists and naval officers.

a creative idea is born in the mind of one man

A creative idea is born in the mind of one man. The idea which finally sparked the initiation of the radar project occurred to L. C. Young of the Naval Research Laboratory sometime in 1930. At that time, while studying transmitter key clicks, he made observations which led him to suggest that the pulse method of echo ranging (used in underwater depth finding) with radio frequency be applied to the detection of aircraft. After much preliminary thinking and calculating, a project was finally initiated March 14, 1934, and the first system was put into operation in December, 1934. While echoes from airplanes were observed, many problems remained. Applied research, having uncovered large areas of ignorance, had to await acquisition of new knowledge through basic research. Basic research work the first half of 1935 at the Naval Research Laboratory made it possible to predict the performance of radar receivers and transmitters. The results were applied to the design of a short time constant, fast recovery, non-blocking radar receiver, and a self-quenched, high-power radar transmitter. A team consisting of R. M. Page and R. C. Guthrie, with help and suggestions from others, then succeeded on April 28, 1936, in putting together equipment which gave satisfactory echoes from airplanes at ranges up to twenty-five miles.

But, as important as was this portion of the development of radar, there was much vital basic research work which had both to precede and to follow before radar could become of military importance. A reasonably complete diagrammatic model of the history of radar is shown in Figure 7. It is a convincing presentation of basic research contributions to the development of aircraft and ship detection systems vital to furthering the missions of the Navy. Further, it is an example of the importance of Navy participation in and coupling with world-wide basic research. Finally, it illustrates successful national and international cooperation in research and development. Numerous contributions were made by the British under the leadership of Sir Robert Watson-Watt, by several National Defense Research Committee laboratories, by Army laboratories, and by several companies in this country such as the Radio Corporation of America, Bell Telephone Laboratories, General Electric, Sylvania, Westinghouse, Raytheon, Philco, and Western Electric. As is so often the case, the knowledge and techniques coming out of the work on radar have made significant contributions to other areas such as communications, computer circuits, scientific instrumentation, television, meteorology, navigation, air traffic and missile control, and radio astronomy.

successful national and international cooperation

The Transistor — A Case History

The transistor, the second example chosen for study, is a recent important spurt or breakthrough in the already exciting field of electronics. This field owes its genesis to the development of the vacuum tube, which permitted amplification of electrical signals. Since World War II it has blossomed into a $7.6 billion industry. Currently, it is being revolutionized by the transistor. This pinhead size wafer of germanium or silicon and its twin brother, the semiconductor diode, perform an ever-increasing number of the functions of the electron tube. With the development of subminiature associated components, they permit electronic systems to be reduced to about one tenth former size. In addition, they require far less power and in their maturity promise trouble-free operation for decades. Recently transistors allowed transmission of the President's Christmas message from a satellite in outer space. Potential civilian and military uses are legion.

While the transistor was invented at the Bell Telephone Laboratories in 1949 by Bardeen and Brattain, working with Shockley, its origins go back at least to 1874, when the phenomenon we now call semiconduction was first observed. The next major step in the development of the transistor came as the result of basic research on electrical conductivity in metals. The knowledge gained was extended by A. H. Wilson into the study of why some materials like selenium, silicon, copper oxide, and silicon carbide conduct electricity about a billion times better than many

insulators and only one millionth as well as metals. This work led in 1931 to modern semiconductor theory, the foundation from which grew the transistor and many other important discoveries.

However, the path was not direct, nor was it simple. Materials 99.9999999% pure had to be made — and a great deal of research went into that. Crystals of high chemical purity had to be produced. The role of surface layers one molecule thick had to be understood. Experiments led to revised theories, and better theories to more refined experiments.

The research effort, in retrospect, was world-wide. Workers from England, France, Germany, Holland, Russia, Spain, and the United States all contributed. In the United States, military research during World War II and basic research, partly military sponsored, at M. I. T., Stanford, Pennsylvania, and Purdue contributed significantly. Techniques, tools, instruments, and isolated facts coming out of the vast atomic energy program also aided the cause. Thus, the transistor did not just happen — the soil was well prepared for the superb, Nobel Prize winning effort of the three Bell Telephone Laboratory scientists. This complex triumph of the inquiring mind is shown in schematic model form in Figure 8. Navy coupling to this field permitted more rapid application of transistors to many pieces of equipment of importance to the Navy.

Importance of the Competent Man

In those fields of science of greatest potential relevance to the missions of the Navy, the surest path to progress in basic research is to secure the services of the most competent scientists within the field. Heavy reliance must be placed upon their judgment. Often they are the only ones possessing the vision or the curiosity to suggest initiation of research projects necessary to the creation of certain new and useful facts. Navy awareness of the importance of seeking out top scientists for participation in its basic research program has proven invaluable.

The rate at which the competent man can contribute to science multiplies rapidly through his guidance and influence on his associates. That this rate can become amazing is shown in our next schematic model, Figure 9. Here is traced the influence on a field of science by I. I. Rabi, one of the many competent scientists selected for support by the Office of Naval Research.

the rate at which the competent man can contribute to science multiplies rapidly through his guidance and influence on his associates

The early work of Rabi and his associates was in molecular beams. At the time this probably seemed remote from any Navy interest. But the inspiration of Rabi and the training he and his associates imparted in basic research and certain experimental techniques were shortly to prove invaluable. Upon the outbreak of World War II Rabi drew many of his students and associates, such as Zacharias, Purcell, Nordsiek, Millman, Schwinger, Kellogg, Kusch, and Ramsey into the M. I. T. and Columbia Radiation Laboratories and proceeded to spearhead the spectacular

Figure 7
Basic Research Foundation Necessary to the Development of Radar
A Schematic Model

Equipment vital to the operating effectiveness of the Navy does not spring forth full grown. The development of radar (*ra*dio *d*etection *a*nd *r*anging) is a case in point. While its widespread use now tends to make us regard it as a relatively simple device, its development was in actuality a long and difficult task. It necessitated an extensive accumulation of knowledge, gained largely since 1800 through the basic research efforts of some of the world's most eminent theoretical and experimental physicists. That some of the important basic work in the embryonic stages of radar was performed at the Naval Research Laboratory is a testimonial to the quality of basic research in the Navy.

This model depicts the major basic research contributions leading to the initial development of radar, the developments resulting from accelerated World War II research on radar and the laboratories contributing, and, finally, some of the other fields of science now benefiting directly from knowledge and techniques developed during the course of research on radar. As can be seen at a glance, a vast complex of knowledge was essential for the birth of this equipment so critical to our national defense.

The model is divided into the four main streams of endeavor which led to radar. Beginning at the top and reading down, these comprise research on microwaves, ultra-high frequency vacuum tubes, amplifiers, and cathode ray oscilloscopes.

Although longer radio waves may be used in radar, microwave research received major emphasis because of the promise it gave for sharper beams and greater accuracy for both range and direction. Following the pioneering work in electricity and magnetism of such great scientists as Faraday, Oersted, Ampere, Henry, and Lord Kelvin, a brilliant breakthrough was made by a young Scottish mathematical physicist. In 1873 James Clerk Maxwell presented a unified theory to account for the known facts about light, electricity, and magnetism. His prediction of the existence of electromagnetic waves was verified experimentally shortly after his death by the German physicist H. Hertz. Marconi excited world-wide interest in short wave radio research by his success in trans-Atlantic communications. This sparked work in radio-wave propagation studies of the ionosphere by Kennelly, Heaviside, and others. It was while performing propagation studies at the Naval Research Laboratory in 1922 that Taylor and Young first noticed reflections of short radio waves from a boat passing in the Potomac River. Ionosphere studies were carried on in 1925 at the Naval Research Laboratory by Breit and Tuve of Carnegie, with the help of Young, using radio pulsers. This led to further work by Young out of which grew the concept for radar using pulse techniques. But much work remained to be done on the generation and reception of microwaves before success was to be had.

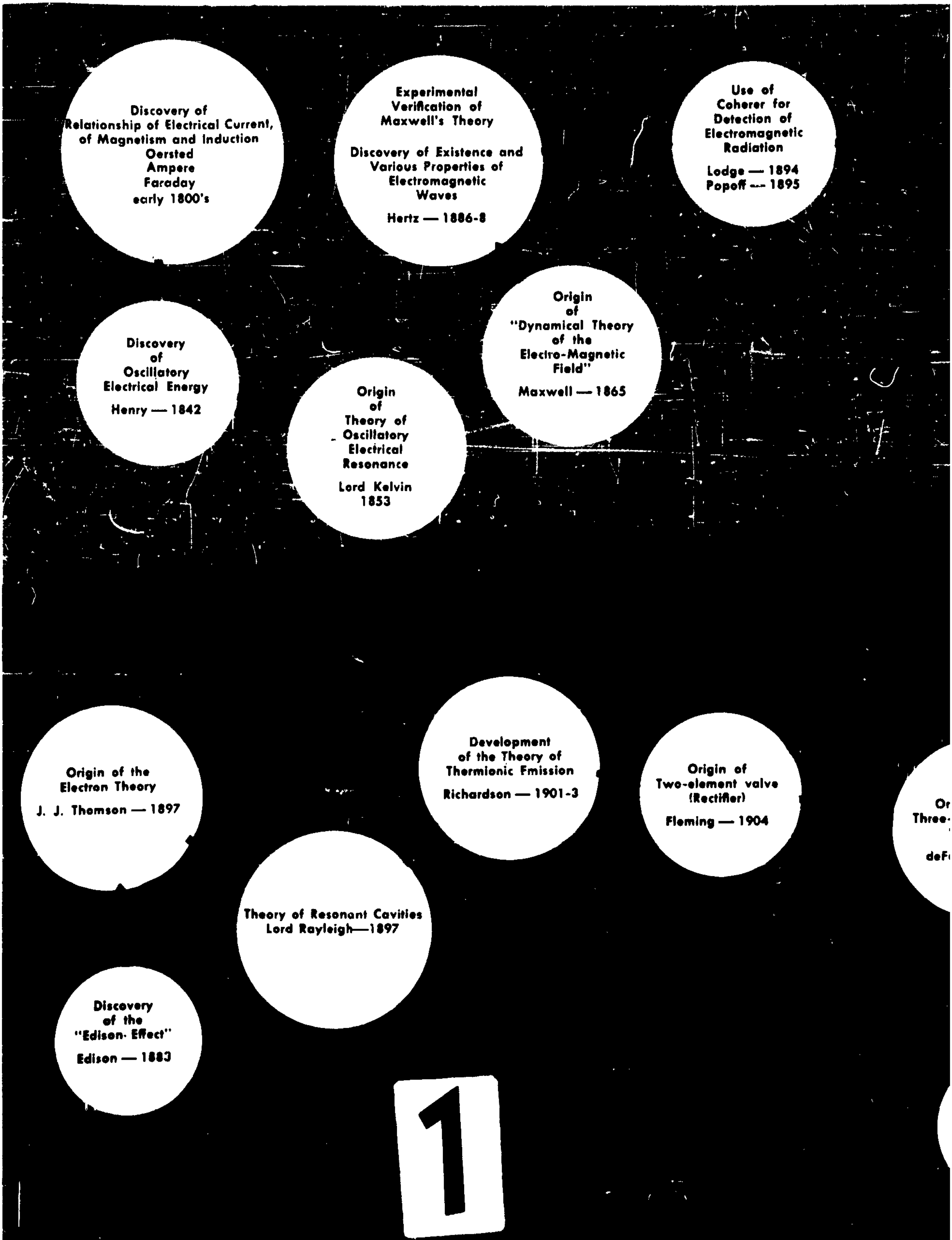
Amplifier development for the detection and amplification of short waves was initiated by Armstrong while in the Signal Corps at the time of World War I. While he had entirely different objectives in mind, his ideas formed the basis for radar receivers.

The single most important component development in microwave radar was the contribution of the resonant cavity magnetron. This was the outgrowth of basic research at Birmingham University in England by a group of physicists under Prof. M. L. Oliphant. Back of this work was a long history of vacuum tube research beginning with Thomas Edison in 1883 and involving such distinguished men as J. J. Thompson, Lord Rayleigh, Richardson, de Forest, Langmuir, Hull, and Okabe. A later development of great significance was the klystron of the Varian brothers and Hansen.

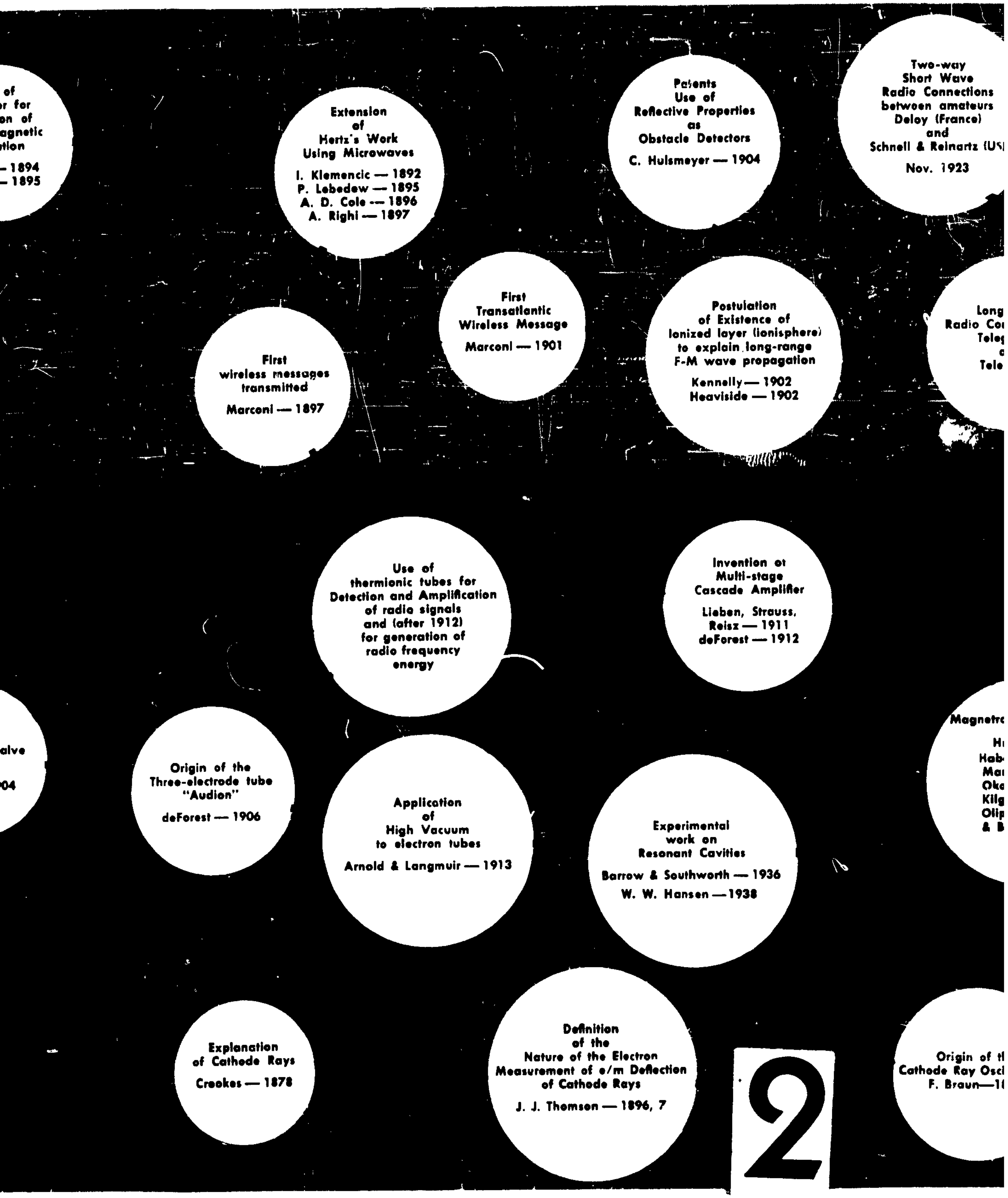
Cathode ray tubes for display purposes had their origin in the work of Braun, building upon the earlier research of others. They were developed into reliable, compact devices through important research by a number of industrial research laboratories such as General Electric and RCA.

The stage for radar was now set. The Naval Research Laboratory made another vital contribution in the duplexer, which permitted the use of a single antenna for transmission and reception. Young, Page, and Guthrie made rapid strides in the pulse technique and detection of aircraft. With war imminent in Europe, the British, under Sir Robert Watson-Watt, the National Defense Research Committee laboratories at M. I. T. and Columbia, and numerous others as listed joined in a prodigious research effort. By 1945 some $3 billion worth of radar was in active military service! It had a significant effect on the outcome of World War II and the revolution of naval warfare.

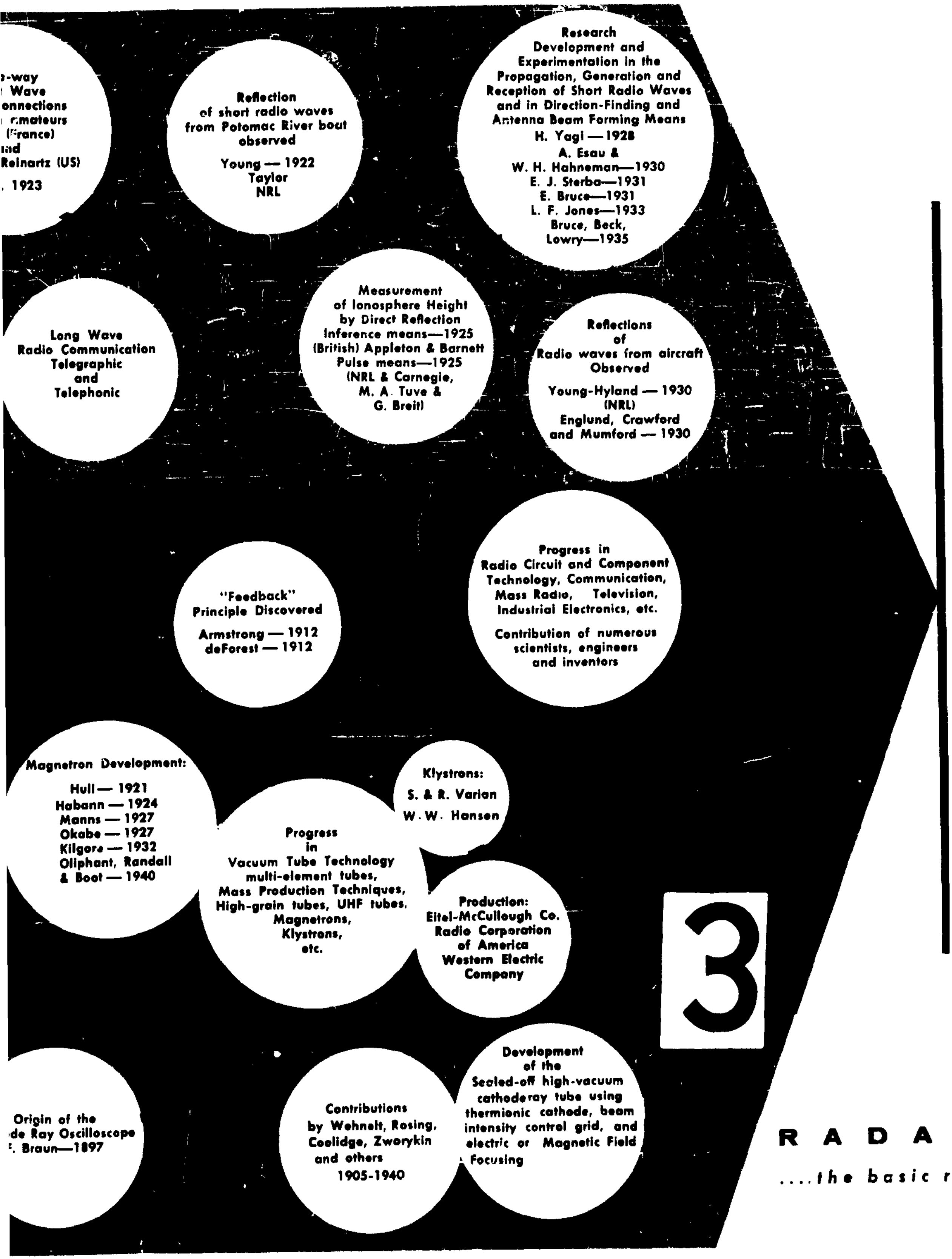
The post-war impact of radar has been no less significant. Knowledge gained has played an important role in furthering numerous fields of basic science, spawning various new industries, and continuing to increase the effectiveness of many weapons systems. And, even greater contributions will be forthcoming.


Discovery of
Relationship of Electrical Current,
of Magnetism and Induction
Oersted
Ampere
Faraday
early 1800's
Experimental
Verification of
Maxwell's Theory
Discovery of Existence and
Various Properties of
Electromagnetic
Waves
Hertz — 1886-8
Use of
Coherer for
Detection of
Electromagnetic
Radiation
Lodge — 1894
Popoff — 1895
Origin
of
"Dynamical Theory
of the
Electro-Magnetic
Field"
Maxwell — 1865
Discovery
of
Oscillatory
Electrical Energy
Henry — 1842
Origin
of
Theory of
Oscillatory
Electrical
Resonance
Lord Kelvin
1853
Development
of the Theory of
Thermionic Emission
Richardson — 1901-3
Origin of the
Electron Theory
J. J. Thomson — 1897
Origin of
Two-element valve
(Rectifier)
Fleming — 1904
Theory of Resonant Cavities
Lord Rayleigh—1897
Discovery
of the
"Edison- Effect"
Edison — 1883

RESEARCH



Extension of Hertz's Work Using Microwaves
I. Klemencic — 1892
P. Lebedew — 1895
A. D. Cole — 1896
A. Righi — 1897
Patents Use of Reflective Properties as Obstacle Detectors
C. Hulsmeyer — 1904
Two-way Short Wave Radio Connections between amateurs Deloy (France) and Schnell & Reinartz
Nov. 1923
First Transatlantic Wireless Message
Marconi — 1901
Postulation of Existence of Ionized layer (Ionisphere) to explain long-range F-M wave propagation
Kennelly — 1902
Heaviside — 1902
First wireless messages transmitted
Marconi — 1897
Use of thermionic tubes for Detection and Amplification of radio signals and (after 1912) for generation of radio frequency energy
Invention of Multi-stage Cascade Amplifier
Lieben, Strauss, Reisz — 1911
deForest — 1912
Origin of the Three-electrode tube "Audion"
deForest — 1906
Application of High Vacuum to electron tubes
Arnold & Langmuir — 1913
Experimental work on Resonant Cavities
Barrow & Southworth — 1936
W. W. Hansen — 1938
Explanation of Cathode Rays
Crookes — 1878
Definition of the Nature of the Electron Measurement of e/m Deflection of Cathode Rays
J. J. Thomson — 1896, 7
2


-way
Wave
onnections
mateurs
(France)
nd
Reinartz (US)
1923
Reflection
of short radio waves
from Potomac River boat
observed
Young — 1922
Taylor
NRL
Research
Development and
Experimentation in the
Propagation, Generation and
Reception of Short Radio Waves
and in Direction-Finding and
Antenna Beam Forming Means
H. Yagi — 1928
A. Esau &
W. H. Hahneman—1930
E. J. Sterba—1931
E. Bruce—1931
L. F. Jones—1933
Bruce, Beck,
Lowry—1935
Long Wave
Radio Communication
Telegraphic
and
Telephonic
Measurement
of Ionosphere Height
by Direct Reflection
Inference means—1925
(British) Appleton & Barnett
Pulse means—1925
(NRL & Carnegie,
M. A. Tuve &
G. Breit)
Reflections
of
Radio waves from aircraft
Observed
Young-Hyland — 1930
(NRL)
Englund, Crawford
and Mumford — 1930
"Feedback"
Principle Discovered
Armstrong — 1912
deForest — 1912
Progress in
Radio Circuit and Component
Technology, Communication,
Mass Radio, Television,
Industrial Electronics, etc.
Contribution of numerous
scientists, engineers
and inventors
Magnetron Development:
Hull — 1921
Habann — 1924
Manns — 1927
Okabe — 1927
Kilgore — 1932
Oliphant, Randall
& Boot — 1940
Klystrons:
S. & R. Varian
W. W. Hansen
Progress
in
Vacuum Tube Technology
multi-element tubes,
Mass Production Techniques,
High-grain tubes, UHF tubes,
Magnetrons,
Klystrons,
etc.
Production:
Eitel-McCullough Co.
Radio Corporation
of America
Western Electric
Company
3
Origin of the
de Ray Oscilloscope
. Braun—1897
Contributions
by Wehnelt, Rosing,
Coolidge, Zworykin
and others
1905-1940
Development
of the
Sealed-off high-vacuum
cathode-ray tube using
thermionic cathode, beam
intensity control grid, and
electric or Magnetic Field
Focusing
R A D A
....the basic r

WARTIME DEVELOPMENT

AIRBORNE RADAR — U. S. NAVY

PULSE ATTENUATOR — 1940
PULSE RADAR — 1941

DETECTION OF NEARBY AIRCRAFT USING PULSE RADAR

YOUNG — 1934 PAGE

BRITISH RADAR EFFORTS

R. WATSON-WATT 1935

DETECTION, GIVING ACCURATE RANGE AND BEARING OF AIRCRAFT AT RANGES OF 18-35 MILES

PAGE — NRL 1936 GUTHRIE

ACCELERATED R AND D OF RADAR COMPONENTS, TECHNIQUES AND SYSTEMS THESE APPLICATIONS WERE OBJECTIVES:

SEARCH FOR AIR AND SURFACE TARGETS
FIRE CONTROL FOR AA AGAINST SHIPS
HEIGHT FINDING AIRBORNE ASW (ANTI-SUBMARINE WARNING)
ASV (AIRCRAFT TO SURFACE VESSEL)
IFF (IDENTIFICATION FRIEND OR FOE)

RESEARCH AND PRODUCTION:

THE NATIONAL DEFENSE RESEARCH COMMITTEE
- M.I.T. RADIATION LAB.
- COLUMBIA MICROWAVE LAB.
- HARVARD RADAR COUNTERMEASURES LAB.

U.S. ARMY SIGNAL CORPS

UNITED KINGDOM LABORATORIES

BELL TELEPHONE LABORATORIES
WESTERN ELECTRIC COMPANY
PHILCO CORPORATION
GENERAL ELECTRIC COMPANY
RADIO CORP. OF AMERICA
WESTINGHOUSE CORPORATION
SYLVANIA ELECTRIC COMPANY
RAYTHEON MANUFACTURING CO.
SPERRY GYROSCOPE COMPANY

SHIPBORNE RADAR

RADAR ON USS NEW YORK USED IN FLEET EXERCISES MARCH 1939
TWENTY PERMANENT US NAVY SHIP INSTALLATIONS — 1940-1941

WARTIME EXPANSION OF RADAR RESEARCH AND DEVELOPMENT.

EXPLOITATION OF NEW FREQUENCIES, TECHNIQUES AND APPLICATIONS.

DEVELOPMENT OF SYSTEM FOR:

GCI-SCI
AIRBORNE INTERCEPTION
BTO (BOMBING THROUGH OVERCAST)
MEW (MICROWAVE EARLY WARNING)
NAVIGATION
BEACONS
MAPPING
RADAR BOMB SIGHT
BOMBER DEFENSE

4

RADAR . . . A CASE HISTORY

....*the basic research foundations necessary for the development of radar*

Figure 7

E DEVELOPMENT

AIRBORNE RADAR — U. S. NAVY

PULSE ATTENUATOR — 1940
PULSE RADAR — 1941

ACCELERATED R AND D
OF RADAR COMPONENTS,
TECHNIQUES AND SYSTEMS
THESE APPLICATIONS
WERE OBJECTIVES

SEARCH FOR AIR
AND SURFACE TARGETS
FIRE CONTROL FOR
AA AGAINST SHIPS
HEIGHT FINDING AIRBORNE ASW
(ANTI-SUBMARINE WARNING)
ASV (AIRCRAFT TO
SURFACE VESSEL)
IFF (IDENTIFICATION
FRIEND OR FOE)

RESEARCH AND PRODUCTION.

THE NATIONAL DEFENSE
RESEARCH COMMITTEE
M.I.T. RADIATION LAB
COLUMBIA MICROWAVE LAB
HARVARD RADAR
COUNTERMEASURES LAB.

U.S. ARMY SIGNAL CORPS

UNITED KINGDOM LABORATORIES

BELL TELEPHONE LABORATORIES
WESTERN ELECTRIC COMPANY
PHILCO CORPORATION
GENERAL ELECTRIC COMPANY
RADIO CORP. OF AMERICA
WESTINGHOUSE CORPORATION
SYLVANIA ELECTRIC COMPANY
RAYTHEON MANUFACTURING CO.
SPERRY GYROSCOPE COMPANY

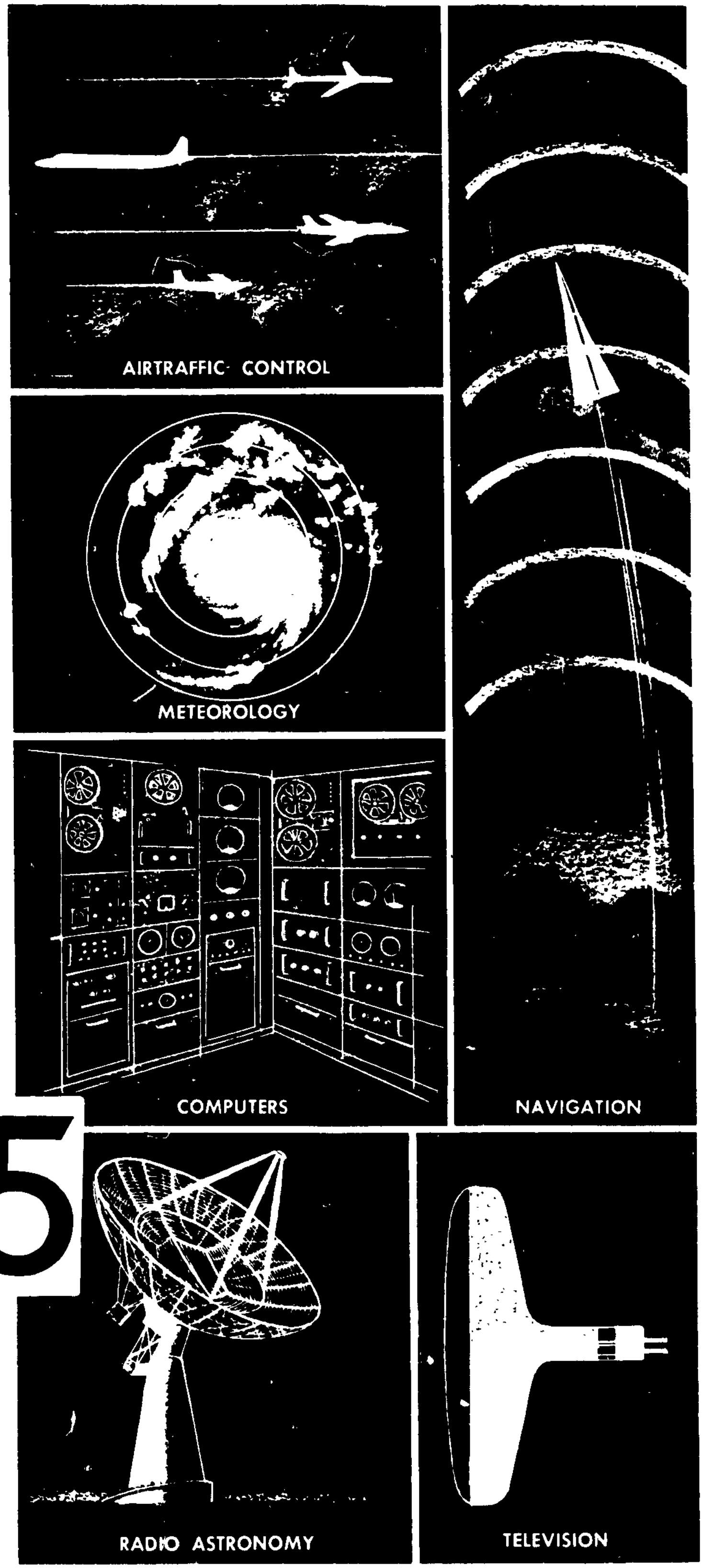
SHIPBORNE RADAR

RADAR ON USS NEW YORK USED IN
FLEET EXERCISES MARCH 1939
TWENTY PERMANENT US NAVY SHIP
INSTALLATIONS — 1940-1941

WARTIME EXPANSION
OF RADAR RESEARCH
AND DEVELOPMENT.
EXPLOITATION OF NEW
FREQUENCIES, TECHNIQUES
AND APPLICATIONS
DEVELOPMENT OF SYSTEM
FOR

GCI-SCI
AIRBORNE INTERCEPTION
BTO (BOMBING THROUGH
OVERCAST)
MEW (MICROWAVE EARLY
WARNING)
NAVIGATION
BEACONS
MAPPING
RADAR BOMB SIGHT
BOMBER DEFENSE

POST WAR DEVELOPMENT



5

ASE HISTORY

cessary for the development of radar

Figure 7

growth of microwave radar. Following the War, with the influx of new apparatus and techniques developed, and a group of new students and associates, the work spread into many new areas Already the basic work of these scientists is bearing fruit in such fields as improved DEW line early warning, missile guidance, and radio astronomy systems. However, the payoff to the Navy from investment in Rabi and associates goes far beyond this. The contributions of the men who appear in one branch or another of Figure 9 are so numerous as to defy estimate. People like Alvarez, Bloch, Estermann, Kellogg, Purcell, Rabi, Ramsey, Townes, Van Vleck, Zacharias, and many others have been instrumental, through service to the President, the Department of Defense, and the Atomic Energy Commission, in the formulation and execution of countless research programs and policies for increasing the naval and military strength of the nation.

The Requirements for Coupling Between Segments of the Research Process

It is now evident that basic research plays an important part in evolving new weapons systems. But why should the Navy have to perform basic research? Why not let the National Science Foundation, or some Department of Defense office, especially established for the purpose, perform all the basic research for the Navy? These questions were often asked during the course of this study. It was the unanimous opinion of leading research directors that so long as there is a Navy with missions as now assigned, it is essential that the Navy participate in basic research. Let us examine the main reasons for this opinion.

it was the unanimous opinion of leading research directors that so long as there is a Navy with missions as now assigned, it is essential that the Navy participate in basic research

An organization which operates in a field dominated by technological obsolescence must have a research and development program if it is to survive. In such a situation the basic research segment of the research process cannot be looked upon as a luxury item which can be separated or cut off at will. On the contrary, it is the life blood of the entire research and development process. Through the circulation of the knowledge and understanding it develops or acquires, basic research is the major coupling force of the process. This is shown diagrammatically in the schematic model in Figure 10. Rapid and effective transmission of new knowledge throughout the over-all program requires the presence of basic research scientists. It is not only their function to develop the new knowledge upon which technological advances are based, but also to acquire additional new knowledge by communication with science on a world-wide basis. It is only they who can understand the work of others who also explore the boundaries of science. It is only they who can seek out through personal contact and through study of world literature the significant new

Figure 8

Basic Research Necessary to the Development of the Transistor

A Schematic Model

The purpose of this schematic model is to illustrate the vast amount of basic research required before it became possible to develop the transistor, a breakthrough of great importance to the Navy in the miniaturization of countless reliable and rugged weapons systems components.

The chart is divided into five rows. The central black arrow represents the main stream of transistor research. The other four rows represent contributing areas of research, all of which were necessary to the continued progress of the main transistor stream. Note in particular the many interactions between techniques, materials research, empirical development, experimental tools, and theoretical work carried out by numerous scientists from many nations before the groundwork was completed for the birth of the transistor.

The earliest semiconductor work dates from 1874 with publications on the electrical properties of lead sulfide, silicon carbide, copper oxide, and selenium. By 1936 a flourishing business of semiconductor power rectifiers existed. However, these developments were largely empirical and could not possibly, of themselves, have led to the transistor. Theoretical guidance and direction from basic research was necessary.

The transistor requires single crystal materials of a chemical purity unattainable twenty years ago — one copper atom in ten billion germanium atoms produces measurable electrical effects. The basic research which permits attainment today of 99.9999999% purity crystals on a commercial scale began with Volmer in 1922. Continued by many investigators, as shown, it led to new concepts of crystal growth, lattice defects, and dislocations, and to the very recent triumph of Dash in the production of dislocation-free silicon single crystals. Simultaneously, the development of ultra-refined chemical analytical tools assisted in the attainment of the crystal purity required.

The development of semiconductor physics began in 1928 with work on the quantum mechanical theory of solids and metals. By 1931 A. H. Wilson brilliantly applied the methods of band theory of metals to formulate the first modern theory of semiconductors. Within a few years Mott and others applied Wilson's results in the development of theories of electrical rectification. The silicon diode was then developed at the Government-supported M. I. T. Radiation Laboratory, following the metallurgical and chemical researches on silicon crystals by Ohl at the Bell Laboratories.

Basic studies on the mechanisms of conduction in semiconductors at the Bell Laboratories and at Purdue, under Navy contract, and basic research on the behavior of semiconductor surfaces at Bell, were the milestones of 1944-48. Finally, in 1948 the first experimental point contact transistor was made by Nobel Prize winning Bardeen and Brattain of Bell. Four years later the theory of the p-n junction was worked out by Shockley (a Nobel Prize winner), and shortly thereafter, Teal and Sparks, also of Bell, made the first junction transistor.

The first devices were fragile, noisy and little more than laboratory curiosities. However, the potentialities of the development so captured the imaginations of scientists that further research was stimulated to a degree seldom paralleled. Transistorized devices are being developed in staggering numbers for both military and civilian use. And soon we will see growing out of the end of this schematic model a whole new series of developments influenced by the knowledge gained — developments in such fields as direct conversion of thermal energy to electrical energy, electronic refrigeration, electroluminescence, parametric amplifiers, solid state masers, and others not now foreseen.

1874 - 1883
EARLY OBSERVATIONS
ON SEMICONDUCTORS G E
Silicon carbide (carborundum)
Lead sulfide (galena)
Copper oxide
Selenium

EMPIRICAL DEVELOPMENT
OF SEMICONDUCTING
DEVICES

THE MAIN STREAM
OF DEVELOPMENT
OF TRANSISTORS

1931
START OF DEVELOPMENT OF
MODERN THEORY OF
SEMICONDUCTORS
A. H. Wilson E

MATERIALS

1

T H E N E T W O R K

INSTRUMENTS
[illegible]

'893
IVATIONS
JCTORS G E
:arborundum)
(galena)
›xide
›m

1929 - 1936
EMPIRICAL DEVELOPMENT
OF COMMERCIAL RECTIFIERS
Copper oxide 1929 US
Selenium 1936 G

1941 - 1943
EMPIRICAL DEVELOP
OF SILICON DIODES FOR
RADAR
MIT Radiation Lal
Western Electric
Sylvania Electri:

931
TART OF DEVELOPMENT OF
IODERN THEORY OF
EMICONDUCTORS
H. Wilson E

1938
THEORY OF RECTIFICATION
Mott E
Schottky G
Davydoff R

CRYSTAL GROWTH
THEORY
Volmer 1922 G
Stranski 1927 G
Kossel 1927 G
Cabrera 19[illegible] S

CRYSTAL GROWTH
TECHNIQUES
Czochralski 1918 G
Bridgman 1925 US
Kyropoulos 1926 G
Stockbarger 1929 US

2

W O R K O F B A S I C R E

TECHNIQUES
High vacuum technology
Furnace technology
Hermetic sealing
Induction heating

1941 - 1945
EMPIRICAL DEVELOPMENT
OF SILICON DIODES FOR USE IN
RADAR
MIT Radiation Lab. US
Western Electric US
Sylvania Electric US

1941 - 1945
EMPIRICAL DEVELOPMENT
OF GERMANIUM DIODES FOR USE IN
RADIO RECEIVERS, COMPUTERS
Siemens 1941 G
Sylvania Electric 1945 US

1948
POINT CONTACT TRANSISTOR
Bardeen & Brattain US

1952
THEORY OF THE P-N
Shockley US

...YSTAL GROWTH

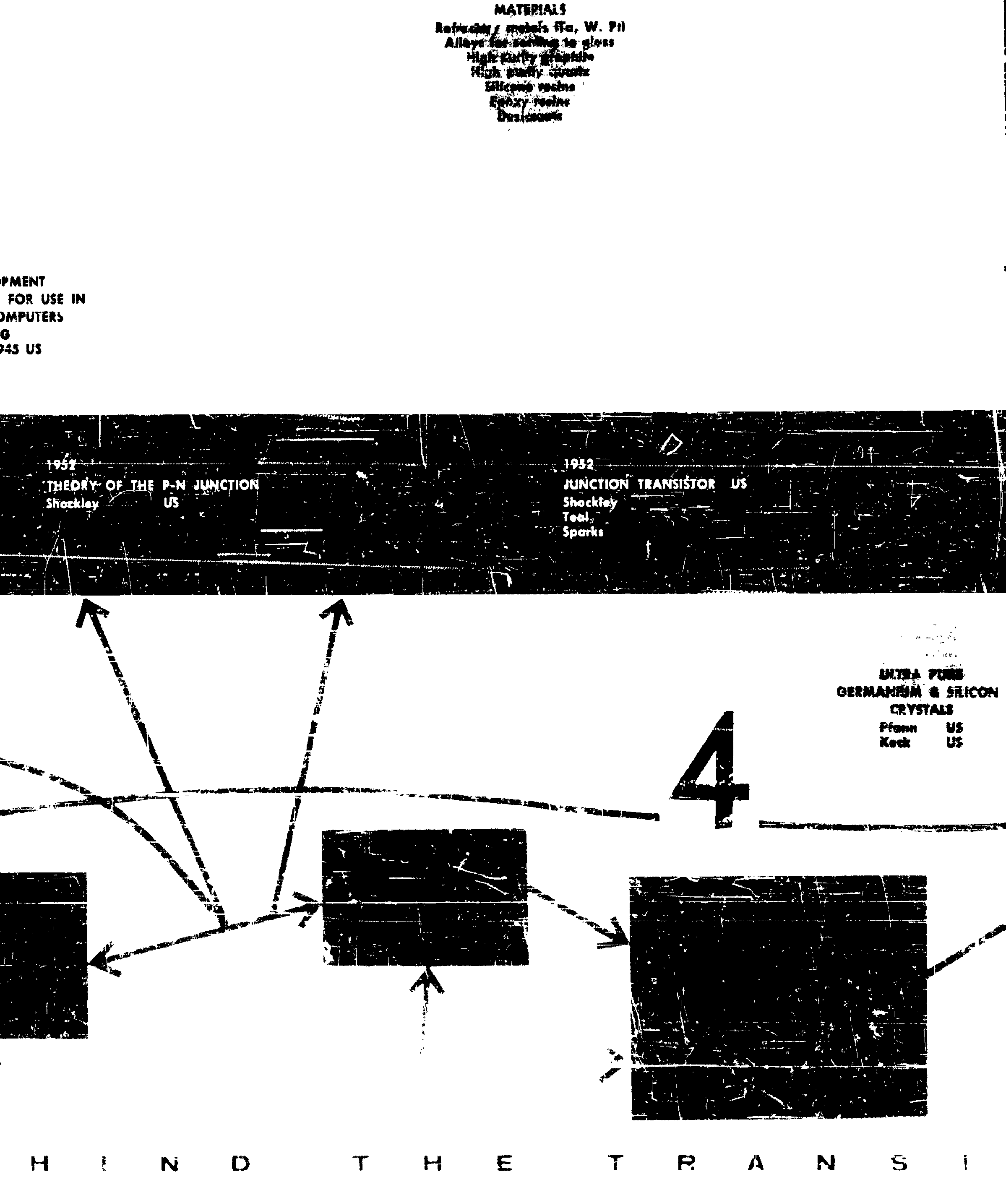
...mann	1918	G
...nen	1925	US
...alous	1926	G
...harger	1939	US

SILICON & GERMANIUM
METAL AND CRYSTALS

Ohl	1935	US
Scaff & Theurer	1949	US
Teal & Buehler	1952	US
duPont		

3

R E S E A R C H B E H I N


MATERIALS
Refractory metals (Ta, W. Pt)
Alloys for sealing to glass
High purity graphite
High purity quartz
Silicone resins
Epoxy resins
Desiccants
PMENT
FOR USE IN
OMPUTERS
G
945 US
1952
THEORY OF THE P-N JUNCTION
Shockley US
1952
JUNCTION TRANSISTOR US
Shockley
Teal
Sparks
ULTRA PURE
GERMANIUM & SILICON
CRYSTALS
Pfann US
Keck US
4
H I N D T H E T R A N S I


COMPUTERS
MILITARY & CIVILIAN RADI
INDUSTRIAL CONTROLS
TELEPHONY & TELEGRAPHY
THE TRANS
... A CASE HIS
... the basic resea
foundations ne
for the developn
transistors
WEAPONS - SYSTEM
FUEL SYSTEMS
RADAR COUNTERMEASURE
DATA PROCESSING
OR US
ULTRA PURE
GERMANIUM & SILICON
CRYSTALS
Pfann US
Keck US
F FRANCE
G GERMANY
H HOLLAND
S SPAIN
5
A N S I S T O R

Figure 9


COMPUTERS
MILITARY & CIVILIAN RADIO
INDUSTRIAL CONTROLS
TELEPHONY & TELEGRAPHY
THE TRANSISTOR
...A CASE HISTORY
...the basic research foundations necessary for the development of transistors
6
WEAPONS SYSTEM
FUEL SYSTEMS
RADAR COUNTERMEASURES EQUIPMENT
DATA PROCESSING

Figure 9

facts appearing on the endless frontiers of science. And it is only they who can transmit the vital information to applied research and development personnel in such a manner as to maximize its utility. This function cannot be performed by liaison men, who quickly lose touch in the rapid march of science, but only by those who continue to participate actively in basic research. The realization of these important facts during a period of accelerating pace in science is the major reason for the growing emphasis on basic research in all the leading industrial research laboratories in the United States (data will be presented in Figure 11).

It is clear that participation by the Navy Department in basic research is essential. Such participation is not "wasteful duplication" on the work of others in industry, universities, or Government. Just the reverse — participation in basic research by each Government department makes more useful the work of every other department. Rapid and effective communication in basic research has cut to a minimum unnecessary duplication. Any duplication is usually undertaken deliberately to corroborate or dispute the results of others. Even then, the method of attack on the same problem by two basic research workers is seldom identical.

participation in basic research by each Government department makes more useful the work in every other department

Time becomes the ruling factor in any race. The present technological race with the Soviet is no exception. To minimize the time cycle from new concept to production of weapons is the primary requirement of national defense today. To accomplish this requires a balanced research program with proper emphasis on, and close coupling between, basic research, applied research, and development. A Navy which failed to participate in basic research in this age would first find itself unable to communicate with the expanding forefront of science, and then find itself unable to evolve the radically new systems which make possible survival.

Supplementary Benefits of Navy Basic Research

The supplementary benefits accruing to the Navy from participation in basic research are centered in research planning and in manpower.

Stemming from the careful initial selection of competent scientists to work on projects in fields related to its missions, the Navy has reaped a growing harvest. Many of these men have become interested in the problems of the Navy to the extent that they might be considered a scientific reserve. They participate extensively with Navy science administrators in the planning and evaluation of research programs; bring to the attention of the Navy interesting projects, and useful results obtained by colleagues; and help single out the bright young scientists who will constitute the leaders of tomorrow. The cooperation developed has also allowed Navy liaison men to perform more effectively their work of seeking out promising projects and scientific information the world over.

Figure 9

The Scientist's Contribution to the Growth of a Field

The Influence of I. I. Rabi

Most directors of research agree that the best method of achieving progress in basic research in a field of science relevant to one's missions is to select a scientist competent in the field, and give him wide latitude as to choice of project and methods of attack within an agreed upon budget. This is because the competent scientist is likely to be the only one capable of visualizing the best means of carrying out the basic research necessary to the discovery of important new facts. This in turn means that during the course of the work there are liable to be individual projects which seem quite remote from one's missions. Thus, the research administrator is sometimes challenged for justification of expenditures of funds, especially if public funds, for certain projects. Here is where sound judgment and patience must be exercised so that a proper decision may be reached.

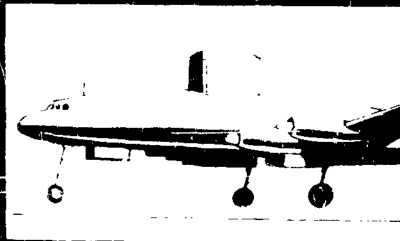
Let us attempt to shed some light on the importance of selecting and backing the competent man by means of a schematic model of an actual case history. The purpose of the model is to trace the extensive contributions to, and influence on, the growth of a field of science over the years by an outstanding basic research scientist working with wide latitude. We have selected I. I. Rabi as the individual for study. He has worked on projects which often seemed at the time to be remote from the missions of the Navy. He has worked sometimes under Navy or Government contracts. And, finally, he has made contributions of great value to the Navy.

The schematic model depicts Rabi as the central figure in the growth of the fields of molecular beams and magnetic resonance, together with those who inspired him, his students, his students' students, and his associates. Short descriptions of important basic research contributions are accompanied by the names of contributors and dates. For the sake of brevity a number of omissions have necessarily occurred, so that the model should be viewed as being illustrative rather than exhaustive.

Rabi's work and influence can be pictured as a rapidly growing and expanding tree. It has as its roots the molecular beam work of Prof. Otto Stern of Hamburg University, who first inspired Rabi while the latter was a student there in 1929. The [illegible] of the tree consists of the further work of Stern and associates, and Rabi and associates in his laboratory at Columbia in the Thirties. The branches represent the tremendous spread of developments to magnetic resonance and allied fields following World War II. (As already noted elsewhere, during World War II Rabi and many of his associates made valuable contributions to microwave radar development at M. I. T. and Columbia.) Many of the branches were directly influenced by Rabi and collaborators and students as indicated by the black and white symbols. Others, such as Bloch and Gorter, were independent contributors as noted by the green color. Work participated in by the Navy is designated by the violet color. A further interesting and important point is that six Nobel Prize winners appear in the model — Stern, Rabi, Bloch, Purcell, Lamb, and Kusch. Rabi, as the central figure, has brought to this field of science a maturity which has left a permanent imprint on world-wide science and technology.

The widespread and growing contributions of Rabi and associates to our knowledge of atomic structure and to the development of new experimental techniques has resulted in many applications of importance to national defense and industry. The applications, at first unpredictable, finally arise as pieces of seemingly remote knowledge become pieced together. Some of these include microwave radar, atomic clocks for timing devices, masers for improved early warning and radio-telescope devices, sensitive magnetometers, and new communications equipment. Further benefits to the Navy continue to accrue in the form of ideas, devices, and systems as a result of association with the frontiers of this field of science. It is now clear why leading research administrators agree that the path to progress in a field of basic research is to back the competent scientist and let him explore for the facts which lead to understanding.

MICROWAVE RADAR



SENSITIVE MAGNETOMETERS

MASERS FOR EAR
AND RADIO TE

A P P L I C

1

1957—SUHL
Ferrimagnetic amplifier

1953—BOHR & MOTTELSON
Collective model of nuclear structure; quadrupole moments

1953—RAMSEY & STUDENTS
Precise measurement of the magnetic moment of the neutron

1953—RABI
Atomic beam magnetic resonan
with optical excited states

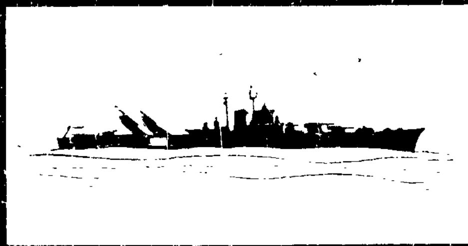
RAMSEY

1951—GORTER
Anti-ferromagnetic resonance

R EARLY WARNING
DIO TELESCOPE

NEW
COMMUNICATION
EQUIPMENT

ATOMIC CLOCKS
FOR TIMING DEVICES



C A T I O N S

2

1958—HARVARD GROUP
Radio astronomy of hydrogen
21 cm. line

1958—BELL & BLOOM; BENDER & ARDITI
Optically pumped frequency standard

1957—J. W. MEYER, SCOVIL
Practical solid state maser

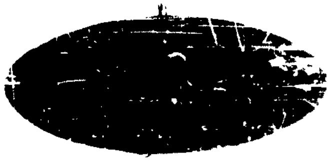
1956—DEHMELT
Optical detection of magnetic resonance

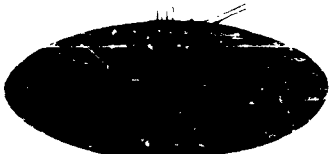
1956—PROKHOROV & BLOEMBERGEN
Solid state maser

1956—PROCTOR & ROBINSON
Coupling of ultrasonic vibrations to nuclear quadrupole moment

1956—FEHER, JEFFRIES & PIPKIN
Dynamic nuclear orientation

1955—ZACHARIAS
Cesium beam atomic frequency standard



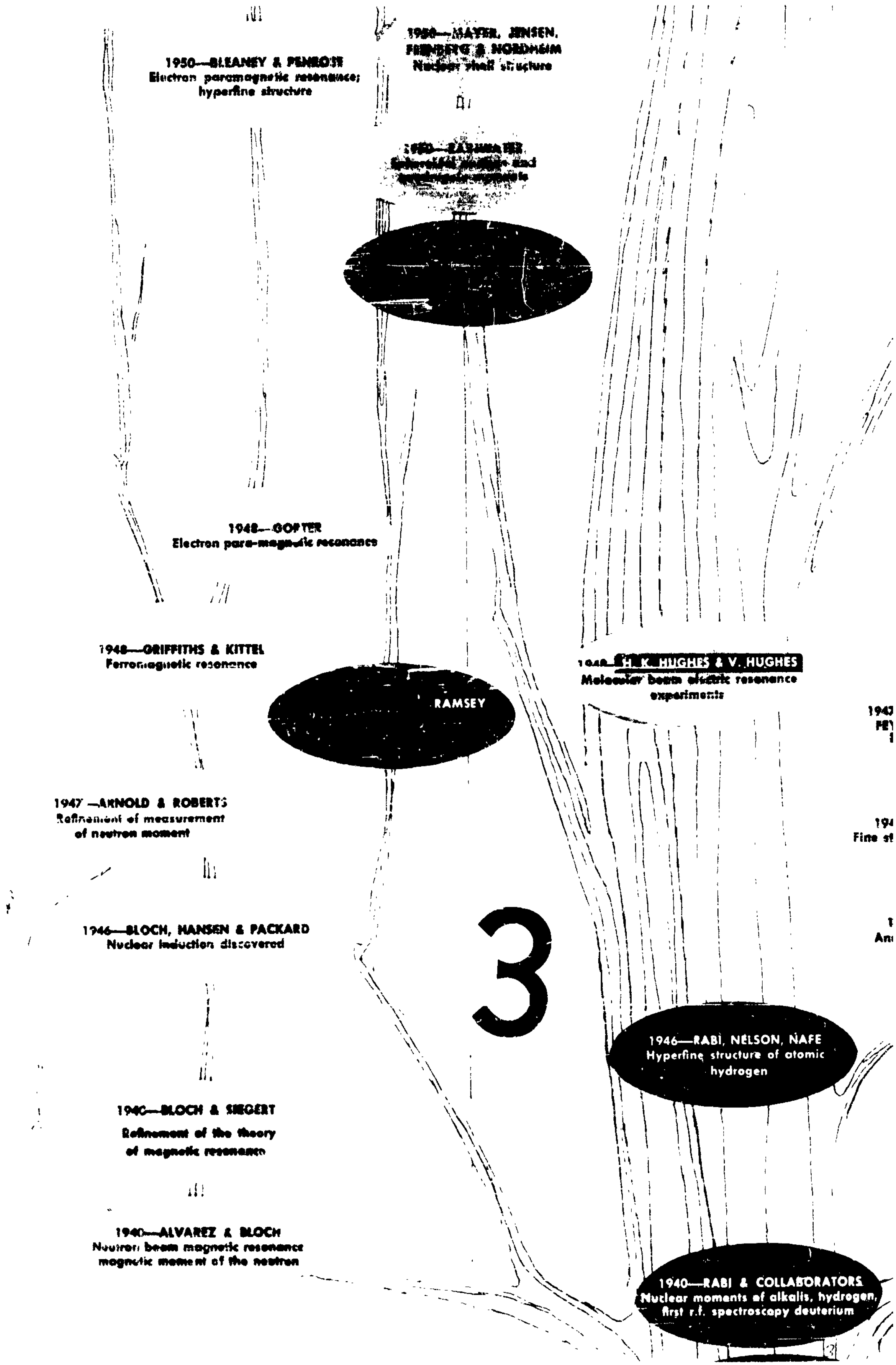


1953—ZACHARIAS
Atomic frequency standard based on atomic beam magnetic resonance

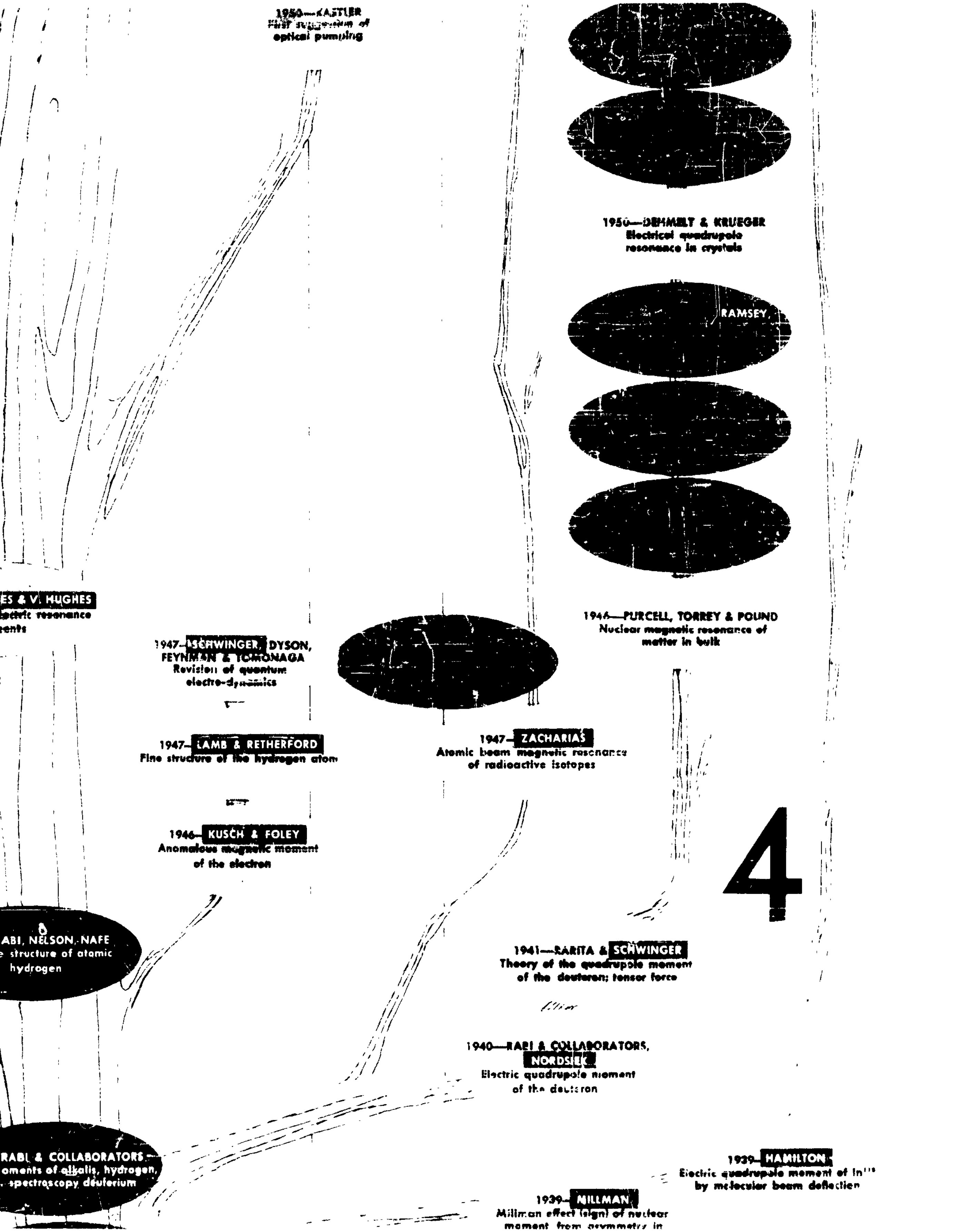
1953—HAMILTON
Atomic beam magnetic resonance of radioactive isotopes

—RABI
gnetic resonance
xcited states

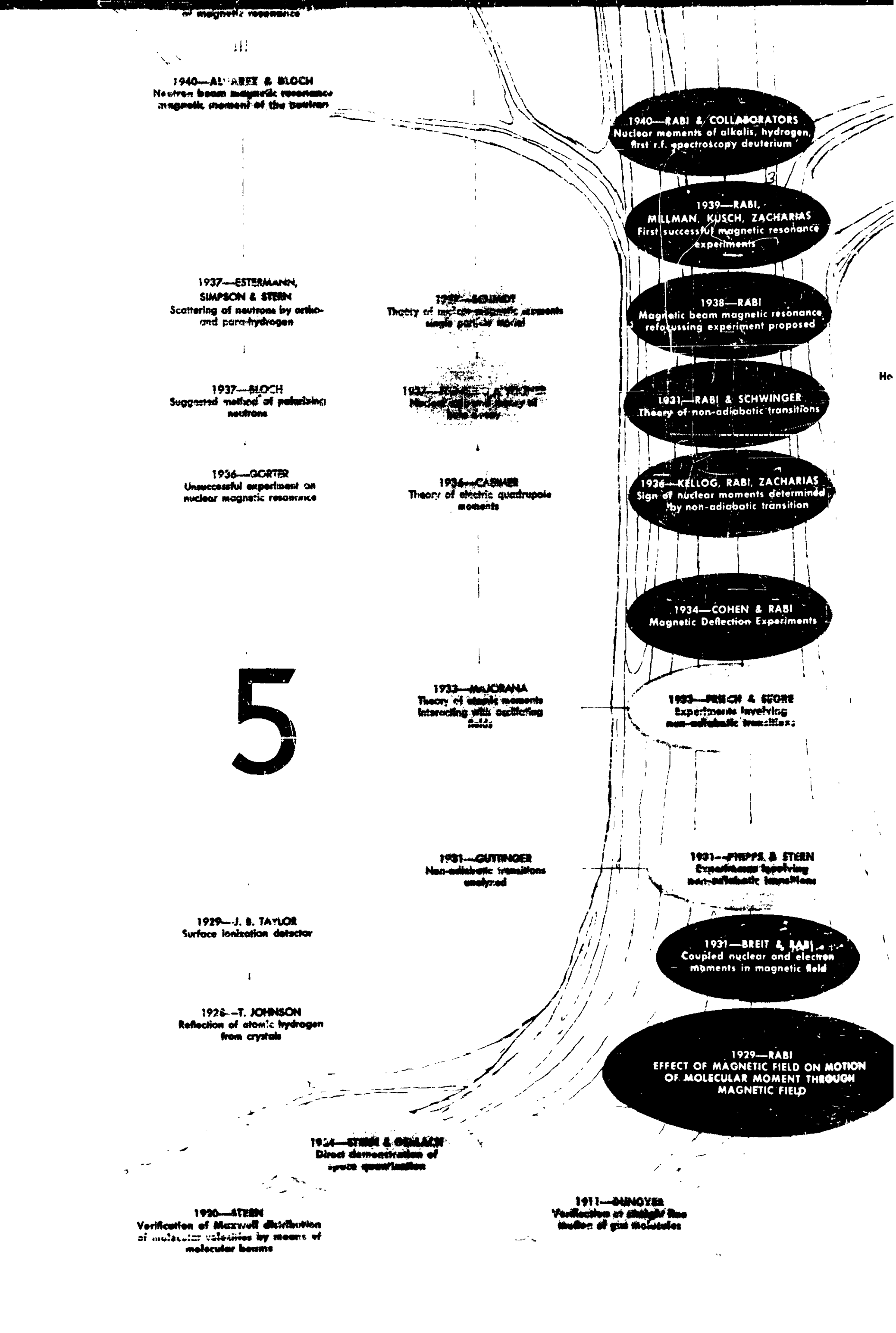
1951—FRIEDBURG & PAUL
Multipole focussing for molecular beams



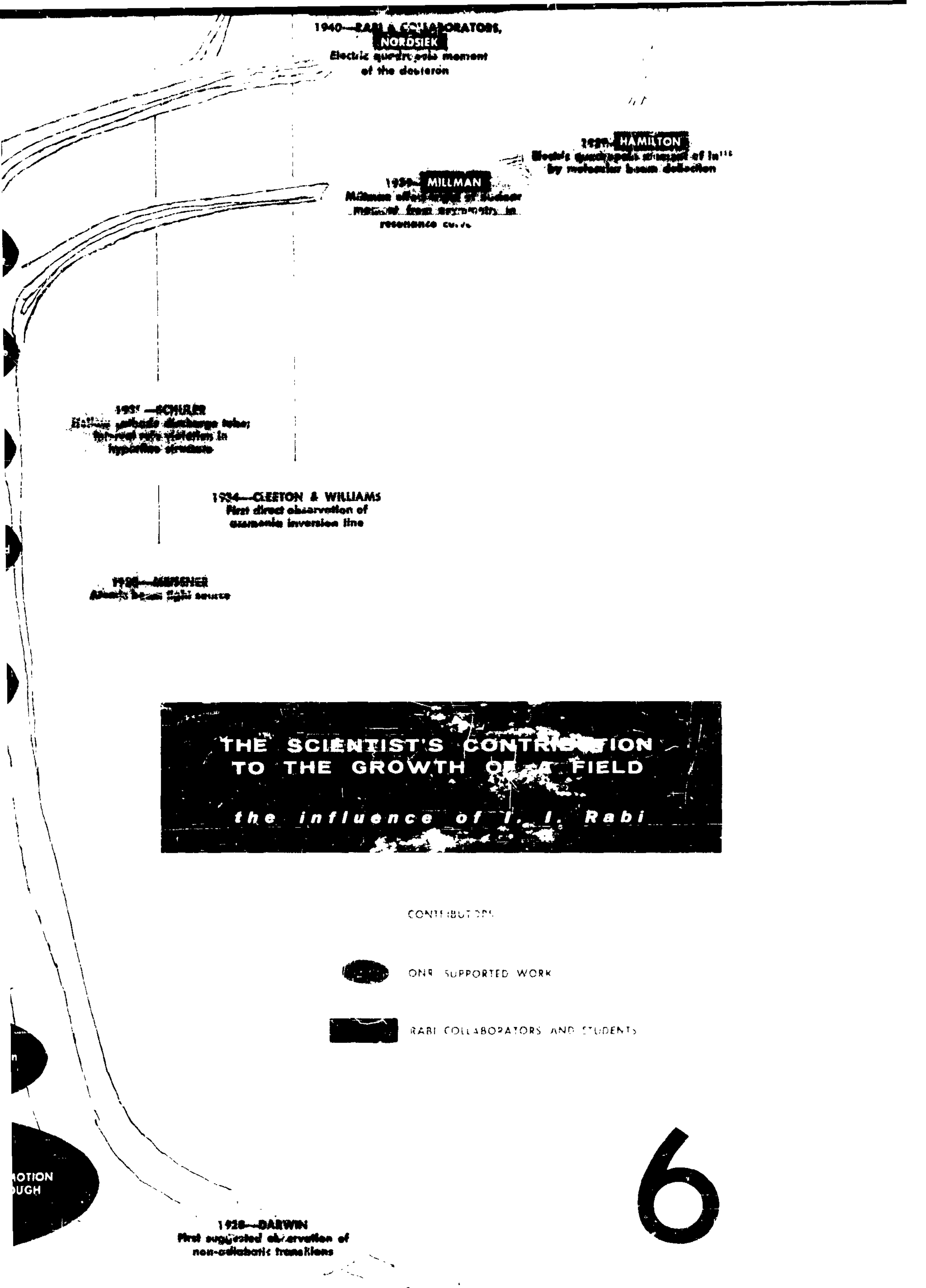
1950—BLEANEY & PENROSE
Electron paramagnetic resonance; hyperfine structure
1950—MAYER, JENSEN, FEENBERG & NORDHEIM
Nuclear shell structure
1948—GORTER
Electron para-magnetic resonance
1948—GRIFFITHS & KITTEL
Ferromagnetic resonance
RAMSEY
H. K. HUGHES & V. HUGHES
Molecular beam electric resonance experiments
1947—ARNOLD & ROBERTS
Refinement of measurement of neutron moment
1946—BLOCH, HANSEN & PACKARD
Nuclear induction discovered
3
1946—RABI, NELSON, NAFE
Hyperfine structure of atomic hydrogen
1940—BLOCH & SIEGERT
Refinement of the theory of magnetic resonance
1940—ALVAREZ & BLOCH
Neutron beam magnetic resonance magnetic moment of the neutron
1940—RABI & COLLABORATORS
Nuclear moments of alkalis, hydrogen, first r.f. spectroscopy deuterium



1950—KASTLER
First suggestion of
optical pumping
1950—DEHMELT & KRUEGER
Electrical quadrupole
resonance in crystals
RAMSEY,
1946—PURCELL, TORREY & POUND
Nuclear magnetic resonance of
matter in bulk
ES & V. HUGHES
ectric resonance
ents
1947—SCHWINGER, DYSON,
FEYNMAN & TOMONAGA
Revision of quantum
electro-dynamics
1947—LAMB & RETHERFORD
Fine structure of the hydrogen atom
1947—ZACHARIAS
Atomic beam magnetic resonance
of radioactive isotopes
1946—KUSCH & FOLEY
Anomalous magnetic moment
of the electron
4
ABI, NELSON, NAFE
e structure of atomic
hydrogen
1941—RARITA & SCHWINGER
Theory of the quadrupole moment
of the deuteron; tensor force
1940—RABI & COLLABORATORS,
NORDSIEK
Electric quadrupole moment
of the deuteron
RABI & COLLABORATORS—
oments of alkalis, hydrogen,
spectroscopy deuterium
1939—HAMILTON
Electric quadrupole moment of In[115]
by molecular beam deflection
1939—MILLMAN
Millman effect (sign) of nuclear
moment from asymmetry in



1940—ALVAREZ & BLOCH
Neutron beam magnetic resonance
magnetic moment of the neutron
1940—RABI & COLLABORATORS
Nuclear moments of alkalis, hydrogen,
first r.f. spectroscopy deuterium
1939—RABI,
MILLMAN, KUSCH, ZACHARIAS
First successful magnetic resonance
experiments
1937—ESTERMANN,
SIMPSON & STERN
Scattering of neutrons by ortho-
and para-hydrogen
1938—RABI
Magnetic beam magnetic resonance
refocussing experiment proposed
1937—BLOCH
Suggested method of polarizing
neutrons
1937—RABI & SCHWINGER
Theory of non-adiabatic transitions
1936—GORTER
Unsuccessful experiment on
nuclear magnetic resonance
1936—CASIMIR
Theory of electric quadrupole
moments
1936—KELLOG, RABI, ZACHARIAS
Sign of nuclear moments determined
by non-adiabatic transition
1934—COHEN & RABI
Magnetic Deflection Experiments
5
1932—MAJORANA
Theory of atomic moments
interacting with oscillating
fields
1931—GUTTINGER
Non-adiabatic transitions
analyzed
1931—PHIPPS & STERN
Experiments involving
non-adiabatic transitions
1929—J. B. TAYLOR
Surface ionization detector
1931—BREIT & RABI
Coupled nuclear and electron
moments in magnetic field
1929—RABI
EFFECT OF MAGNETIC FIELD ON MOTION
OF MOLECULAR MOMENT THROUGH
MAGNETIC FIELD
Reflection of atomic hydrogen
from crystals
Direct demonstration of
space quantization
1920—STERN
Verification of Maxwell distribution
of molecular velocities by means of
molecular beams
1911—DUNOYER
Verification of straight line
motion of gas molecules


NORDSIEK
of the deuteron
HAMILTON
by molecular beam deflection
MILLMAN
resonance curve
1934—CLEETON & WILLIAMS
First direct observation of
ammonia inversion line
THE SCIENTIST'S CONTRIBUTION
TO THE GROWTH OF A FIELD
the influence of I. I. Rabi
ONR SUPPORTED WORK
RABI COLLABORATORS AND STUDENTS
1928—DARWIN
First suggested observation of
non-adiabatic transitions
6

An important corollary benefit of the Navy research program is the training of basic research scientists. It supplements the primary effort of the National Science Foundation in this field in areas of direct interest to the Navy. An interesting example of this is the strengthening of our capabilities in underwater sound. Through basic research contracts with universities it was possible in five years to more than double the number of trained men in this portion of physics so vital in anti-submarine warfare. Many who leave the field of basic research following their training also become of key importance to the Navy. For example, a brief study of the weapons systems program of the Navy indicated that applied research and development in most projects is so complex that the great majority of those chosen to become project leaders are persons whose training was in basic research. They combine the rare ability to understand the broad problems, to plan programs and to make key contributions.

an important corollary benefit of the Navy research program is the training of basic research scientists

Fields of Science Related to the Missions of the Navy

Because of the great diversity of Navy missions, its interests necessarily extend into most of the major fields of science. The following is a list of these fields approximately in order of current Navy basic research expenditures:

Physics
Astronautics and Aeronautical Engineering
Material Sciences
Electronics
Mechanics
Medical Sciences
Biology and Biological Sciences
Oceanography
Chemistry
Geography
Psychology
Operations Research
Meteorology
Astronomy and Astrophysics
Mathematics
Combustion
Earth Physics

This cannot be viewed as a priority list as costs of performing basic research vary with the field of science. Any detailed monitoring of the basic research program of the Navy is a large and continuing task, obviously far beyond the scope of this assignment. Some general impressions gained, however, are considered worth mentioning.

Figure 10

Coupling Between Segments of the Research Process

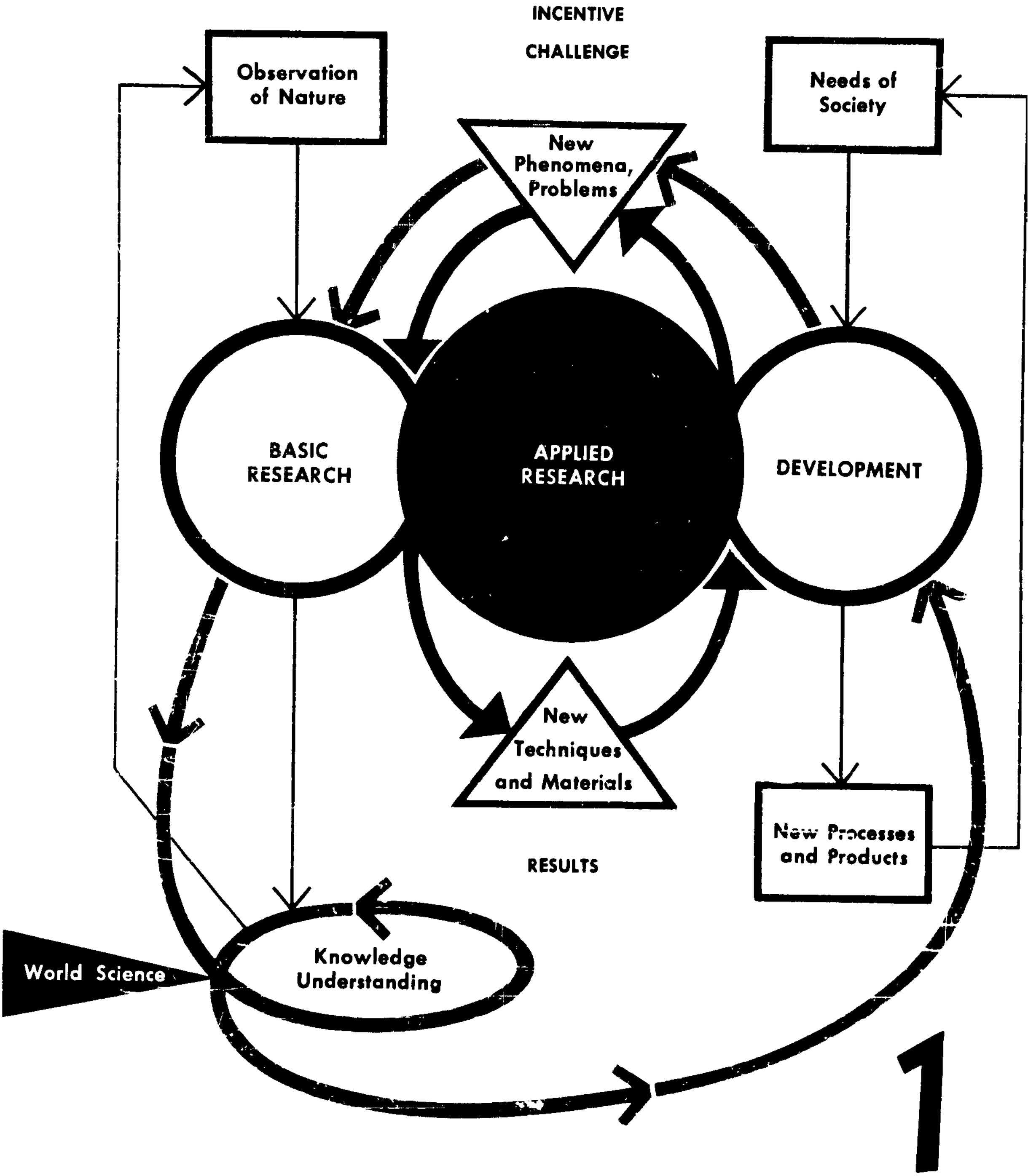
Basic Research Provides the Life Blood

This diagram presents a schematic model of the research process. Inspection shows immediately that the collective use of human intelligence in research involves a system of interconnecting circuits.

A characteristic of each of the three major segments, basic research, applied research, and development, is an ability to feed upon itself as well as upon other segments. Ideas wherever originated within the system continually generated new ideas. This is shown by the several cyclic lines. Perhaps the best proof of this phenomenon of regeneration or feedback is the tremendous growth rate of science. This same feedback principle is easy to visualize in the case of the military program in that each new measure immediately sparks the necessity for development of a countermeasure.

But the strongest characteristic of the research process is the requirement for coupling between the three segments. The key to this coupling is the transmission and use of the knowledge and understanding which springs from basic research. This circulation of new knowledge gained through basic research can be thought of as the life blood of the research process, as indicated by the red line. It is only by establishing such flow to permit easy and understandable communication with the frontiers of science on a world-wide basis that a healthy and progressive research program can be maintained That this can be accomplished by the Navy only by actually participating in basic research, not through dependence on a separate agency, is agreed upon by all leading research administrators. Participation in basic research injects into a program basic scientists in a manner which permits them to tap the important reservoir of world science and to catalyze the progress of the entire effort. This is one key to minimizing the time cycle from new research discovery to production.

A few examples of the benefits already accruing to the Navy by the close coupling of its basic research with applied research and development are shown in brief below the model.



THE MACHINERY OF THE RESEARCH PROCESS

. . . . basic research provides the life blood

Figure 10

1.	Basic Research on Infrared Radiation →	Infra-red Emission from jet aircraft engine →	IR seeker on Sidewinder missile	
2.	Basic Research in Solid State Physics →	Solid State Maser →	Low Noise, high gain Amplifier which operates in microwave Region →	First application of mase to Naval Research Lab radio telescope gives 16-fold improvement in sensitivity
3.	Basic research in physiological optics, low level photometry visibility of stars in twilight atmospheric transmission, spectrum of solar radiation. →	Problem of computing and predicting the visual detection of orbiting satellites →	Surface finish for satellite (Vanguard); criteria for location of observation stations.	
4.	Need for radiation counter which is not sensitive to thermal failure at low temperatures, capable of high counting rates, undamageable by electrical overloading. →	Basic research on gas discharges and electronic and ionic processes in gases. →	Development of halogen-quenched geiger counter. →	Detection of radioactivity
5.	Basic research on spectral emissivity of flames. →	Technique for optical measurement of temperature of varying flume. →	Application to temperature measurement along exhaust flame of a rocket engine.	
6.	Difficulties in Operation of certain electronics equipment →	Research in Ionization Propagation by Meteors. →	Research shows that Ionization from meteor trails could be used for extended-range communications	
7.	Basic research on effects of ultrasound on biological tissues →	Development of ultrasonic irradiator →	Treatment of diseased brain tissues.	
8.	Basic research in chemistry of large molecules →	Solid rocket propellants →	Fuel for Polaris missile.	
9.	Basic Research Program on methyl derivatives of Hydrazine →	Unsymmetrical DiMethyl Hydrazine →	High energy fuels for liquid rocket engines	
10.	Operational difficulties in aviation instruments and ordnance fuses →	Need for better oils →	Fundamental investigation of films absorbed on surfaces →	Synthesis of better clock oils
			← Contribution to fundamental knowledge of mechanism of wetting, and relation to composition of solids and liquids.	
11.	Geophysical data on thermal properties of land and sea masses →	Basic research on rate of heat flow from interior of earth; heat exchange between ocean and land; characteristics of Continental Shelf & Arctic Coastal Plain; changes in climate due to geothermal gradients. →	Application to location of air-strips on land and sea-ice; construction of Distant Early Warning Systems.	

2

12. Radio wave propagation is affected by distribution of water vapor in atmosphere. → Effects on Navy communications and radar → Important to know variations of water vapor in the atmosphere over the transmission path → Development of instrument to make rapid measurements of water vapor in atmosphere → Understanding of effects of water vapor in radio propagation

Basic research on optics of upper atmosphere → Basic research on Lyman-Alpha radiation at high altitudes → Development of instrument to make rapid measurements of water vapor in atmosphere

13. Basic research on metallurgical fine structure → Introduction of fine substructural network in metal by working and annealing treatments → Ultra high-strength steels → Military applications: aircraft landing-gear.

14. Basic Research in mathematics → Theory of Highspeed Computing Machines → Electronic Computers for Scientific Computation → Use of computers to handle information in air defense systems (ONR research tool computer. Whirlwind used for Sage Continental Defense System)

15. Understanding the mechanism of super-cavitating flows → Basic for utilization of super-cavitating flows → Design of pumps and propellers for high-speed operation. → Application to rocket fuel pumps, propellers for high speed ships and torpedoes.

16. Basic research in pathology → Problem of toxicity of submarine hydraulic fluid → Rapid solution to toxicity problem

Basic research in physiology → Problem of toxicity of submarine hydraulic fluid

Basic research in organic chemistry → Problem of toxicity of submarine hydraulic fluid

Basic research in biochemistry → Problem of toxicity of submarine hydraulic fluid

17. Research in variations of human behaviour, under varying conditions of acceleration, pressure and temperature → Evaluation of display and control systems for aircraft → Design of aircraft controls and weapons systems to optimize performance and safety of human operators.

18. Research in shallow water sound propagation → Determination of acoustic impedance and sound velocity in a variety of bottoms, using correlation technique with noise → Basic understanding of propagation of sound in the sea. → Design and use of acoustic [illegible]es, and underwat[illegible] detection and ranging systems.

19. Research on velocity of sound in sea water → Precise measurement velocity of sound in sea water → Knowledge of the liquid state → Sonar systems using deep propagation paths; prediction of shock wave propagation in underwater explosions.

3

The listed fields of science are logically of interest to the Navy. In them the areas of opportunity in assisting the furtherance of the missions of the Navy are legion. Better understanding of such things as the elementary particles, nuclear forces, chemical bonds, mathematical techniques and tools, atmospheric physics, dynamics of oceans, man-machine complexes, solid state, plasma characteristics, and hydro-dynamics offers a multitude of possibilities in new systems of warfare much beyond our ability to predict.

However, the Navy obviously cannot and should not cover all aspects of these fields of science. In its planning the Navy must take what amounts to two cuts in establishing a program. The first cut involves a general allocation of effort between fields, depending on Navy interest, and the extent to which others are already providing support. The second cut gets down to the type of detail wherein the science administrators, working with the advice of competent scientists, must have authority to place their bets on the basis of the competence of the investigator and the relevance of the project. Within each field of science listed there will be three types of project choices. One type will be of direct Navy interest. A second type will be of interest, but less obviously so. A third type will be of only speculative interest, but nevertheless one with which the Navy should be in communication lest a breakthrough of vital importance occur. A classic example of the latter was early Navy work in nuclear physics which ultimately permitted more rapid utilization of nuclear power for ship propulsion. It is not possible to define firm boundaries as to Navy interest because of the unpredictability of basic research results and the complex inter-relationships between fields of science. Thus, trying to look into the future intelligently is the thing which causes every research administrator to lose sleep. This is when wisdom to make the proper choice, patience to await results, and strength to justify expenditures become so important.

While existing Navy program planning, selection of contractors, coordination with other agencies, and communication are generally good, there is always room for improvement. In the planning of research it should be possible to make use of additional scientists and research administrators from universities and industry. In this way more information can be obtained on programs under way in many fields of science, on new basic research policies of others to permit comparison with those of the Navy, and on new techniques of planning and budgeting. This should strengthen the basic research program and project selection, and tend to eliminate any unnecessary overlap with expanding industrial basic research. Improvement in communications should also be possible. In this area the Navy faces a difficult problem, since more than one half of its basic research is, quite properly, performed by contract. This means that a special effort must be made to closely couple this work with the applied research and development programs in Navy contractor and subcontractor laboratories. Improved communications require continued

emphasis on personal contact through meetings arranged by science administrators and liaison personnel, and improved means of recording and distributing information in readily accessible form. Adequate travel funds will be a necessity.

the appropriate Navy laboratories are well aware of the importance of basic research

The appropriate Navy laboratories are well aware of the importance of basic research in maximizing their contributions to furthering the missions of the Navy. In this regard they are outstanding among the Department of Defense Laboratories. The Naval Research Laboratory, which compares favorably with many top industrial laboratories in devoting 20-25 percent of its funds to basic research, is responsible for about 30 percent of all papers published by the 49 Department of Defense establishments publishing in 16 selected scientific journals. Other highly rated Navy laboratories such as the Naval Ordnance Laboratory, Naval Ordnance Test Station, and Navy Electronics Laboratory were also found to be publishing significantly. The knowledge generated in the basic research work of these and other Navy laboratories, and knowledge gained through their contacts with basic research performed elsewhere, have contributed significantly to improved Navy effectiveness.

However, in discussions of the Navy program with the top scientists in many of the Navy laboratories, it was evident that they believed the Navy budgetary and administrative policies with respect to basic research were too limiting. Among these men so well aware of the serious problems in national defense today there was general agreement that the Navy should place much greater emphasis on participation in, and communication with, that segment of science responsible for the initiation of our major technological innovations. As will be seen in the next section, there is much justification for their considered position.

An Approach to Establishing A Proper Level of Navy Participation in Basic Research

Two methods of approach were selected in attacking the problem of establishing a proper level of Navy participation in basic research.

The first involved seeking out the judgment of many people competent in research and its administration, and responsible for setting the basic research budgets within their own organizations. The policies and practices of large segments of industry and government were investigated. New data on research and research personnel were collected and analyzed.

The second involved a mathematical analysis of the research process in an attempt to develop a method of predicting for a given project the optimum division of effort to be devoted to basic research, applied research, and development. If this can be done, it should then be possible to project the analysis to cover the Navy broadly.

At the beginning of the study it became painfully evident from the diversity of opinions encountered that the definition of basic research was a matter which had to receive detailed attention. For purposes of this project the official Department of Defense definition, as previously recorded, was adopted. While this definition, as that of any general concept, is necessarily broad, it was found to have rather wide acceptance. The problem, however, lies in the interpretation of the definition. Argument over the meaning of basic research definitions has gone on for some time, as is evident both in reports of Congressional hearings and reports of meetings of research administrators. Unless the definition is interpreted similarly, it would be impossible to obtain comparative data on basic research budgets and policies from Government, industry, and university sources.

It was decided to attack this problem by ignoring the debate over the meaning of definitions, and proceed directly to a study of the output of

basic research. The concept that the output of all meaningful basic research is almost invariably represented by scientific papers published in recognized scientific journals was found to have almost universal acceptance by research personnel and administrators.*

cases of secrecy in basic research are infrequent and merely delay, rather than prevent, publication

Cases of secrecy in basic research are infrequent and merely delay, rather than prevent, publication. Therefore, it was reasoned that if there is widespread consistency in the interpretation of the definition of basic research, there should be a correlation between the number of people claimed to be performing basic research in Government, industry, and university laboratories, and the number of papers originating from each of these sources appearing in selected scientific journals.

In the exploration of this thought, data previously collected by the National Science Foundation were used to calculate the number of basic research workers claimed in 1953-54 by Government, industry, and university laboratories. The number of papers originating from each source was then obtained from inspection of a selected sample of thirteen recognized scientific journals covering various major fields of science. The publication count was for the year 1957, permitting a reasonable elapse of time for research and publication. The results obtained are recorded in Table I.

The strong correlation shown in Table I permits the conclusion to be drawn that policy with respect to the interpretation of what constitutes basic research, and freedom to publish, is remarkably consistent nationwide. With the growing tendency for more liberal publication policies on the part of industry, there is indication that the correlation will become even stronger.

This gratifying and significant finding had two important results. First, it meant that comparable data on basic research policies and budgets could be obtained from various sources. Second, it permitted a rough check to be made so that the validity of data from a given source might be determined, when desired, merely from a simple literature count. Such checks applied to a number of Government and industrial laboratories further confirmed the conclusions drawn from Table I.

Comparison of Navy and Industry Basic Research Allocations

industry represents the second largest source of basic research funds

Industry represents the second largest source of basic research funds in the United States. Since many corporations each year face budget problems of a complexity, if not magnitude, comparable with the Navy, it was decided to compare the practices of the two with respect to basic

* Assistance on this subject from Dr. John C. Fisher of the General Electric Co. is gratefully acknowledged.

research. Inquiries were directed to a large segment of our more technically based industry. Cooperation was excellent. Through discussion and correspondence information was obtained from thirty-three leading corporations representing the source of almost one fifth of the nation's and one half of industry's total basic research funds. Information on the Navy was obtained through the Office of Naval Research.

TABLE I

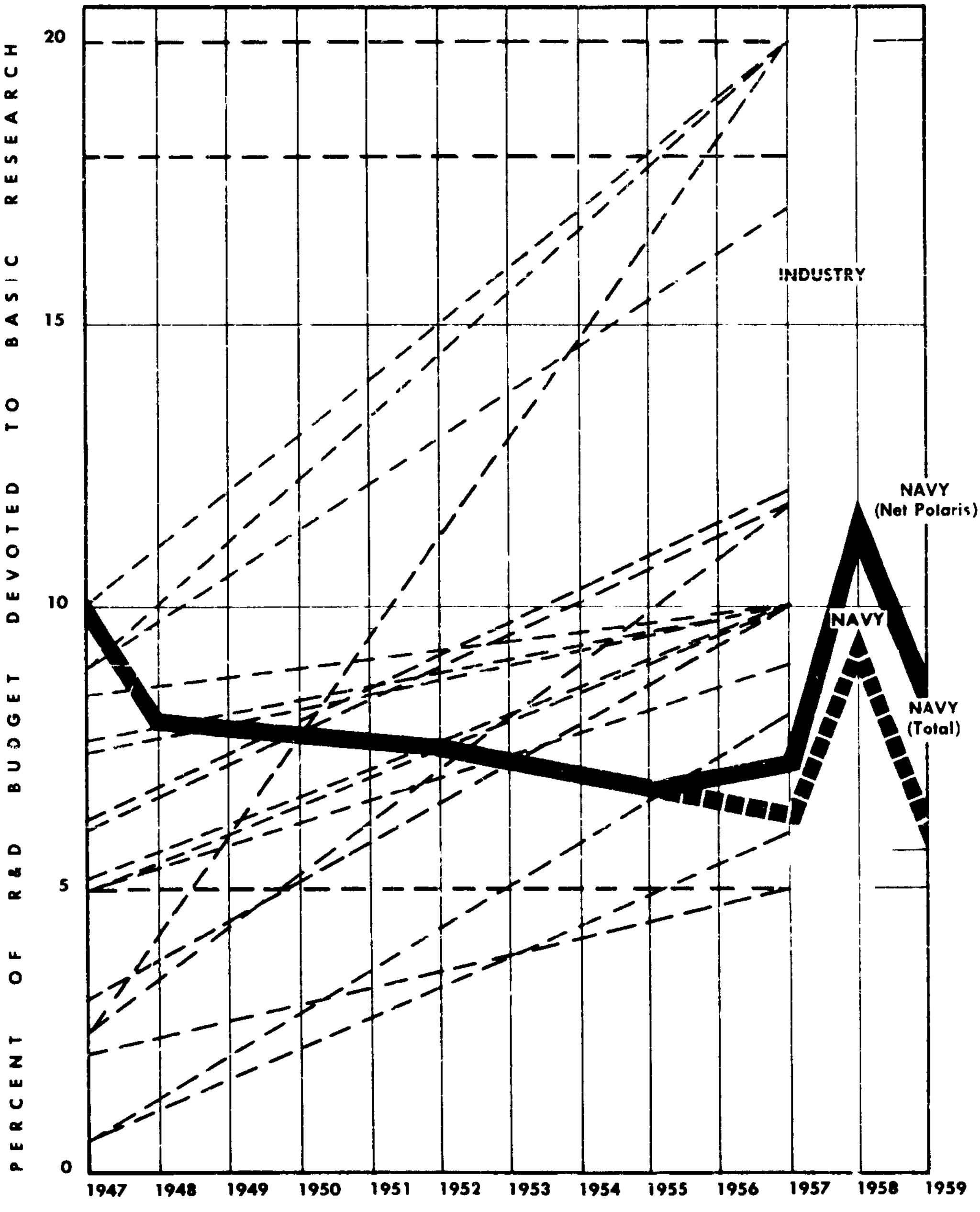
Relation Between Number of Basic Research Workers Claimed and Output of Basic Research as Measured by Scientific Papers Published

Type of Organization	Distribution of Basic Research Workers, 1953-1954	Distribution of Papers Published in 13 Selected Scientific Journals, 1957
Government	7%	9%
Industry	27%	19%
University and Non-Profit Institutions	66%	72%

For a comparison of the practices of the Navy with those of industry, it was decided to study the trends over the decade 1947 to 1957. The earlier date marked the beginning of major Navy basic research expenditures, and 1957 represented the last full year for which data were available from industry at the time of this undertaking. Data were collected in terms of dollars, or, where such data were confidential, in terms of allocation of funds. By this is meant the percent of the research and development budget devoted to basic research.

The industry information was obtained from those executives responsible for allocation of funds within the over-all research and development budget as approved by the Board of Directors. The funds considered were solely corporate funds, exclusive of any Government research contracts. Engineering expenditures of a type not normally included in the research and development budget were excluded from the data obtained.

The data obtained are extremely interesting. A graph presenting the percent of the total research and development budget devoted to basic research in 1947 and 1957 by the Navy Department and by nineteen leading corporations is shown in Figure 11. From this it is readily apparent that, while the Navy compared very favorably with industry in 1947, when it devoted 10 percent of its research and development budget to basic research, industry has since outstripped the Navy in emphasis on basic research. This has come about largely as a result of the growing realization by industrial management of the importance of participating in, and communicating with, that portion of science which creates the



PROPORTION OF RESEARCH AND DEVELOPMENT DEVOTED TO BASIC RESEARCH

. . . a comparison between the navy and twenty leading industrial corporations

Figure 11

knowledge and understanding from which burst forth our major technological advances. Put in another way, applied research and development tend to proceed more rapidly, and at lower cost, when adequately backed by basic research.

applied research and development tend to proceed more rapidly, and at lower cost, when adequately backed by basic research

That the Navy operates today in a fiercely competitive field having a high technological obsolescence rate, is generally agreed upon. Some 80-100 percent of ships, aircraft and missiles scheduled for purchase in 1959 were of types not in existence in 1955. Thus, for more meaningful basic research guidelines, the Navy should be compared with corporations in high technological obsolescence rate industries. Two of the most successful corporations in five such industries (chemical, petroleum, communications-electronic, pharmaceutical and materials) were selected for study. These ten corporations had a minimum of 10 percent and a maximum of 20 percent of their research and development budget allocated to basic research. The average was about 16 percent, or more than double the present Navy figures of 6-8 percent.

Other figures confirm the faster pace of industry. Fourteen top corporations in these same industries released to us dollar figures in order to permit comparisons with the Navy. Between 1947 and 1957 these corporations tripled their research and development expenditures and increased basic research expenditures by a factor of 4.5. In the same period the Navy doubled its research and development expenditures, but increased basic research expenditures by a factor of only 1.5. This smaller increase in basic research expenditures by the Navy was essentially offset by reason of the fact that total cost per scientist increased about 50 percent during this same period. This figure of a 50 percent increase has been the experience of a number of laboratories, and is more meaningful than the lesser increase in Consumer Price Index, which has been used in some comparisons.

But the Navy cannot be directly compared with any one corporation or group of corporations. Missions, competitive situation, size, and complexity are all different. Nowhere is success more important today than in military technological advance. The consequences of being second best in national defense today represents a risk far greater than faced by any corporation. Recognizing this, we requested a number of leading research directors to project their experience and judgment into consideration of the problem of Navy participation in basic research. Thirty-three were approached, all representing corporations considered to be outstanding in their particular fields. Of these, sixteen believed they had sufficient knowledge of the Navy to be willing to express an opinion. They were unanimous in their belief that the Navy should increase its participation in basic research. The majority thought that the complex nature of the mission of the Navy was such as to command an allocation of some 15-20 percent of the research and development budget to basic research. This represents a substantial increase over the current Navy allocation of 6-8 percent.

the consequences of being second best in national defense today represents a risk far greater than faced by any corporation

research cannot be turned off and on without producing disruptive effects on program and organization

Although numerical ratios are often cited as measures of desirable levels of research and development effort, it must be understood that these are to be taken as general guidelines and not as "magic numbers" or rigid criteria. While ratios of 1-5 percent of the sales dollar devoted to technical work are generally quoted, no company, we believe, establishes an over-all figure for its research and development budget on such a basis. Actually the practice is to evaluate the need for technical effort on recommended projects or areas according to the desired rate of progress, and then to total project requirements as a preliminary over-all budget. This is reviewed with top management, and any readjustments made by changing emphasis on individual projects. Admittedly there is usually a historical trend in the budget which might make it appear that somewhat fixed ratios are used. The hazard of the fixed ratio is that it might cause fluctuations inimical to sound research planning. Research cannot be turned on and off without producing disruptive effects on program and organization.

If one considers specifically the information obtained from industry regarding the percentage of total research and development budget devoted to basic research, it should be noted that these percentages were not set arbitrarily at fixed levels, but have been reached over the years on the basis of judgment as to optimum balance between the need for new knowledge and the effort required to apply the accumulation of knowledge to the company's business.

The level of basic research effort suggested as appropriate to the needs of the Navy is, therefore, to be taken as a general guideline. It is implicit that the budget be erected on the basis of careful evaluation of the need for new knowledge, area by area, and that as increased effort appears to be justified, the total of the sub-budgets would be gradually increased in this step-wise fashion.

One of the concepts most often encountered when research policy was discussed with industry was that one should never do less basic research than his strongest competitor. With this in mind, it is desirable to assess briefly the Soviet situation, since the Navy must play its part in meeting this challenge. The best estimates which could be made within the scope of this report indicate the following:

> Soviet political leaders are credited by a number of investigators with a greater knowledge of science than ours, and a greater appreciation of its role in furthering the progress of a nation. This disparity is not confined to Government circles; indeed, the percentage of ministerial-rank persons having a scientific or technical education is higher than that found at the management level of most top corporations in the United States. In fact, the USSR appears to be the first nation to fully appreciate the importance of science. This is evident in

many areas such as the vast effort in technical education, the high percentage of gross national product expended on research and development, the important stature accorded scientists in the Soviet society, and the large program to collect, translate, and disseminate scientific publications.

The current policy of the Soviet Government appears to be to direct the development of its science and technology toward achieving military, political, and economic supremacy over the United States. Back of the recent technological successes by the Soviet is a program of basic research staffed by approximately the same number of scientists as that of the United States. Whenever such a situation occurs the nation which places more emphasis on a particular field of science will tend to lead in that field. While over-all comparisons have many shortcomings, it appears that currently the United States leads the USSR in most areas of physics, mathematics, medicine, and chemistry; is on a par in aviation and space medicine, metallurgy, combustion, theoretical physics, meteorology, and oceanography; and is behind in physical chemistry and many areas of geophysics.

The important problem, however, is the future. Currently the Soviet is training persons capable of performing basic research in science at a rate approximately 50 percent greater than the United States, while essentially keeping abreast of the United States in granting doctorate degrees in other fields. Thus, the Soviet potential is increasing relative to ours at an alarming rate.

This brief account of the competition presents a real challenge to the nation. The outlook is not entirely black. But to meet the challenge will require increased wisdom both in the planning and administration of research and development to make most effective use of our resources, and in the training of additional men of higher quality.

Some of the Problems of Increasing Navy Basic Research

Should the Navy decide to increase its participation in basic research, at least two problems will arise. One will be availability of scientific manpower, and the second will be improved methods of budgeting. Therefore, consideration was given to these matters.

At present it appears possible to increase the basic research participation of the Navy. This opinion is based on the outcome of a study by the Coordinating Committee on Science of the Department of Defense. It showed that for 1957, if funds had permitted acceptance of all meritorious proposals, the Department of Defense basic research effort in outside contracts could have been increased 70 per cent. This figure is probably reasonably accurate, because the increase in proposal submissions which would occur with increased availability of funds would essentially offset the tendency for certain research organizations to suddenly become understaffed through acceptance of all outstanding proposals. The factor of large capital equipment items, which could have a substantial effect on budget and personnel, has been omitted from this particular listing of meritorious proposals. (It is understood that this is the subject of a separate study.) In addition, a rough approximation indicates that an increase in basic research effort of about 10 percent could be made now in Navy laboratories. The situation of having additional personnel currently available will not persist long, because research and development activities are expanding about 10 percent per year, whereas the number of scientists is increasing at a rate of only 5 percent per year.

An interesting approach was made to the study of basic research manpower, involving once again the counting of papers appearing in selected scientific journals. This technique permits two important findings not well covered in previous manpower studies. First, it shows who is performing basic research. One rather disturbing discovery is that only 20-30 percent of all physicists and chemists who obtain doctor's degrees publish basic research papers following thesis submission. It is not known as yet whether this is caused by attraction to other positions having more appeal or reward, or by a lack of ability or interest in basic research. Second, paper counting, although obviously not the whole story, provides a rough means of evaluating basic research scientists. For example, physicists are rated by physicists and other scientists by election to the National Academy of Sciences, as a Fellow of the Physical Society, or as a member of the Physical Society. Study of the records of all physicists earning doctors degrees in 1936, 1941, 1946, and 1951 indicated that Fellows of the Physical Society publish at a rate about ten times that of non-Fellows, and members of the National Academy of Sciences at about twice the rate of Fellows of the Physical Society.

more men will have to be trained, and perhaps more motivated to remain in basic research

It is obvious that if the Navy, other parts of Government, industry, and universities are to increase basic research in any substantial way in the future, more men will have to be trained, and perhaps more motivated to remain in basic research. The latter is not a simple decision, as it has previously been shown that basic research trained men are extremely useful in other occupations. Another method of extending the work of basic research scientists is to provide them with better equipment and more technical assistants. Experience gained in the past six years makes

this appear to be a promising avenue. Whether to try to motivate more persons interested in post graduate work to shift into the sciences is a point of debate. There has been no change in the ratio of science doctorates to other doctorates granted since 1932. Many people believe no effort should be made to upset this relationship, which stands today at roughly 30 percent physical sciences, 20 percent life sciences, and 50 percent doctorates in other fields.

A final problem on manpower has to do with hiring and retaining top flight personnel for Navy laboratories. Since basic research requires excellent personnel, Navy laboratories, to be effective, must be permitted to operate with more competitive salary and administrative policies.

The matter of budgeting for basic research is complicated by the necessity for planning on a long-term basis, while budgeting and operating on an annual basis. Planning basic research involves estimating the time needed to form the research team, perform experiments, and analyze and publish the results. The over-all time required for this process, as measured by the current average life of Office of Naval Research projects, is 5.1 years. This figure varies with the size of the project, those of less than $10,000 averaging 3.5 years, those of $10,000-$30,000 per year averaging 4.8 years, and those greater than $30,000 per year averaging 6.5 years. The Office of Naval Research has been able to obtain the budgetary mechanisms for long-term financing of basic research. Its funds are made available by means of a no-year (available until expended) appropriation, which helps solve most if not all of the legal and contracting problems involved in long-term financing. In addition, it has Congressional approval of the policy of long term advance financing of research projects. Under this policy projects are financed for an average of two years with individual contracts funded as far in advance as five years. The use of these budgetary tools is, however, strictly limited by the amount of funds made available each year and by the uncertainty of subsequent years' appropriated amounts. Stiff competition for funds is offered by current fleet readiness, hardware, and personnel requirements.

The budget problem is one of broad national interest, involving many agencies in addition to the Navy Department. The solution can be obtained by providing better understanding of the role of basic research to serve as a basis for coordinated budget planning by the Executive Branch and Congress. Since the Office of Naval Research has had so much experience with this problem, it could serve as an excellent testing ground for improved procedures.

A Proposed Mathematical Model of the Research Process

The invention of a new device or process is essentially a synthesis, a putting together of principles, relationships, and facts. These building

blocks of invention themselves all had to be discovered. Many of them were discovered so long ago that they are now taken for granted, such things as the wheel and the screw, such materials as iron and glass. Others are more recent, but new enough so that we realize that they have not always been available, for example the electric motor. Still others are so new that the public is not generally aware of them.

No matter how many or how few these principles, relationships, and facts may be, one thing is certain: the invention could not have been made until all were discovered. There is therefore an earliest date at which any invention could have been made. No matter how great his genius, Leonardo da Vinci could not have invented television. This is not to say that inventions cannot be conceived before their time. Jules Verne conceived a missile fired around the moon, but in his day the actual construction and firing of such a missile was quite impossible.

in every invention there exists a key fact

In every invention there exists a key fact, the last to be discovered of all the facts, relationships, and principles which were necessary before the invention could be made. The date of the discovery of this key fact is the earliest date at which the invention could have been made. Some inventions have been made very quickly after the discovery of the key fact, others have been made long after, but no invention was ever made before the discovery of its key fact.

Research is the process by which these principles, relationships, and facts are discovered. Without research, invention must come to a stop, for there is a finite number of ways in which a given body of knowledge can be applied. This is not to say that the stoppage would be instantaneous, for it takes time for inventions to be made, but without research the rate of invention would grow slower and slower until it fell to zero.

This decay in the invention rate may be thought of in the following way. At any instant of time there exists a body of knowledge, a set of facts, etc., which have been discovered. Some of these may be useless, and will never play any part in any invention. Some may be applied once, others many times. Among these a few are key facts, the discovery of which makes an invention possible.

The number of facts required for an invention is ordinarily very large, but only one is the key fact. While a certain fact may be used in a large number of inventions, it is most probable that it is not the key fact in any of these. There is some chance that it may be the key fact in one invention, but, as a matter of experience, very unlikely indeed that it is the key fact in more than one.

Coming back to our body of knowledge, this body contains a certain number of key facts, corresponding to an essentially equal number of possible inventions. If no new knowledge is added by research, these represent all the inventions which can be made.

We can symbolize this process by comparing it to a two stage chemical reaction

$$A \longrightarrow B \longrightarrow C^*$$

Where A represents the key facts not yet discovered, B represents the key facts which have been discovered, but not yet applied, and C represents the final applications. The first step is the research process of finding the key facts. The second step is the process of invention.

The chemical analogy suggests, and the theory of search developed during World War II reinforces, the idea that the rate of the first step is proportional to the effort put into the process and to the number of undiscovered facts. Similarly, the rate of the second step should be proportional to the effort put into it, and to the number of discovered, but unapplied, facts. Thus the first rate should be of the form

$$k_1E_1A$$

and the second of the form

$$k_2E_2B$$

where E_1 and E_2 are the respective efforts, and k_1 and k_2 are the two constants of proportionality.

The constants k_1 and k_2 are measures of the relative ease with which the two processes can be carried out. If k_1 and k_2 are equal, the two processes are equally easy. If $k_1 = 10\,k_2$, it is 10 times as easy to find a fact as to apply it, and so on.

To find the proper balance of effort between the two steps, it is clearly necessary to find a way of determining these "ease factors." One approach to this is by the analysis of past experience. Let us suppose that during the development of a field the effort put into each of these two processes is held at a constant ratio. It can then be shown that the number of facts in the three categories A, B, and C should change with time in the way shown in Figure 12.

If it were possible to observe all three of these curves the analysis would be relatively simple. Unfortunately data of this kind are hard to obtain. The only data we have been able to find are a few cases, which give only the C curve. These few cases, however, are in excellent agreement with the prediction of this theory. Furthermore, they indicate a ratio of k_1/k_2 in the neighborhood of 2. That is to say, it is twice as easy to discover a fact as to apply it.

It would be risky in the extreme to draw the conclusion that this ratio is universal. It may very well be that this ratio varies widely from one field of research to another. Nevertheless the data do suggest that the

* **Detailed development of the mathematical model is given in Volume II.**

general lines of the theory may be correct, and that the "ease factors" are at least of the same order of magnitude.

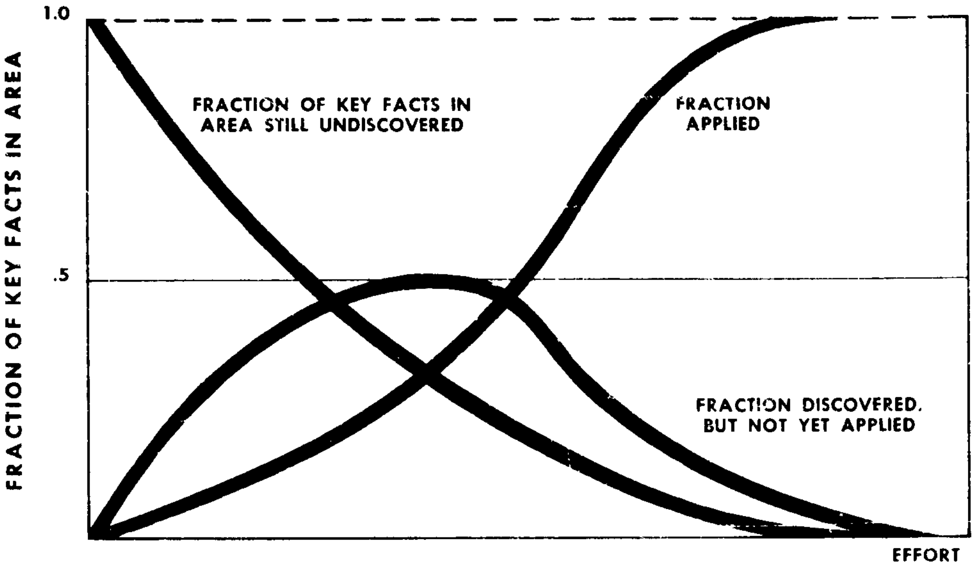
If this theory of the research process can be accepted, it now becomes possible to study the problem of the correct distribution of effort between the two steps. It is clear that both kinds of effort are necessary; the question is: how should a given total effort be divided?

If too much effort is put into the first step, and too little into the second, the result will be the discovery of a large fraction of the key facts, but the application of only a small fraction of those discovered. If too much effort is put into the second step, and too little into the first, only a small fraction of the key facts will be found. While a large fraction of the discovered facts will be applied, the number of applications will be small because the number of discovered facts is small.

The general situation is shown in Figure 13. The three curves in this figure represent three levels for the total amount of research effort put into the development of a field. Each curve shows how the total result of the effort (measured as the number of inventions) changes as the distribution of the effort between basic research (step 1) and applied research and development (step 2) is varied. If the total effort is small, the best result is obtained when the two efforts are equal. As the total effort is increased, the position of the maximum shifts. How this shift takes place depends on the "ease factors," k_1 and k_2. The curves in Figure 13 are drawn for a case in which k_1 is larger than k_2. In this case the shift is toward less basic research and more applied research. If k_2 were greater than k_1, the shift would be in the opposite direction.

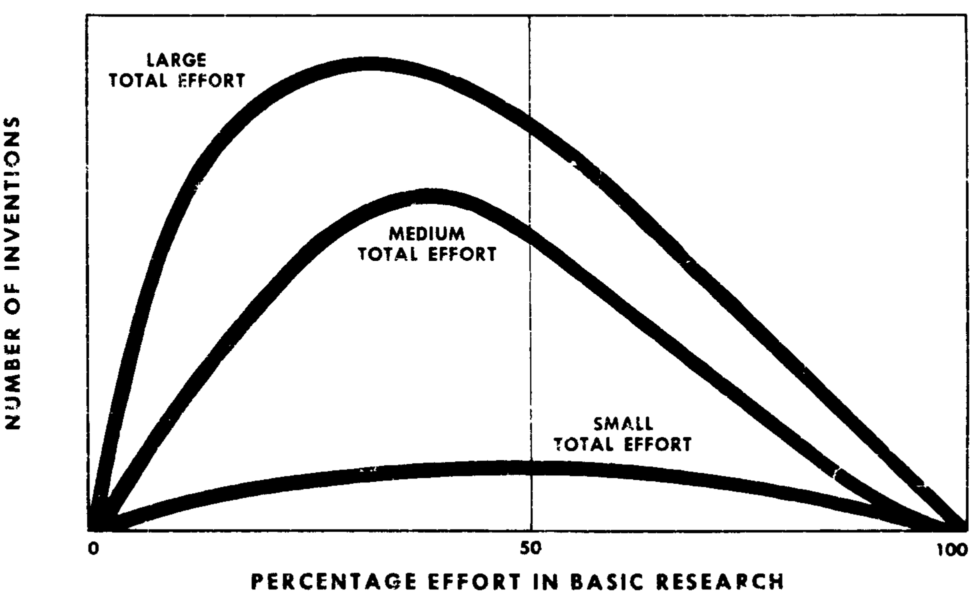
Figure 14 shows these shifts in greater detail. The curves show the way in which the optimum distribution of effort changes as the total effort is increased. The curves are plotted for the three cases $k_2 = 9\,k_1$, $k_1 = k_2$, and $k_1 = 9\,k_2$. Taking the curve $k_1 = 9\,k_2$ as an example, the curve shows again that for small efforts, the effort should be equally divided between basic and applied research. As the total effort is increased, the fraction which should be devoted to basic research decreases. It should be noted that the horizontal scale in this figure is the fraction of the possible inventions which are made. The right hand side of the figure therefore represents an infinitely large effort.

If the present indications are to be believed, the actual ratio of k_1 to k_2 is about 2. If this is the case, the optimum fraction of basic research in a large program to develop a field should be in the neighborhood of 30%. This suggests that a larger effort should be placed in basic research than is now the case. We hope that in the future additional data will become available so that this indication can be tested further.



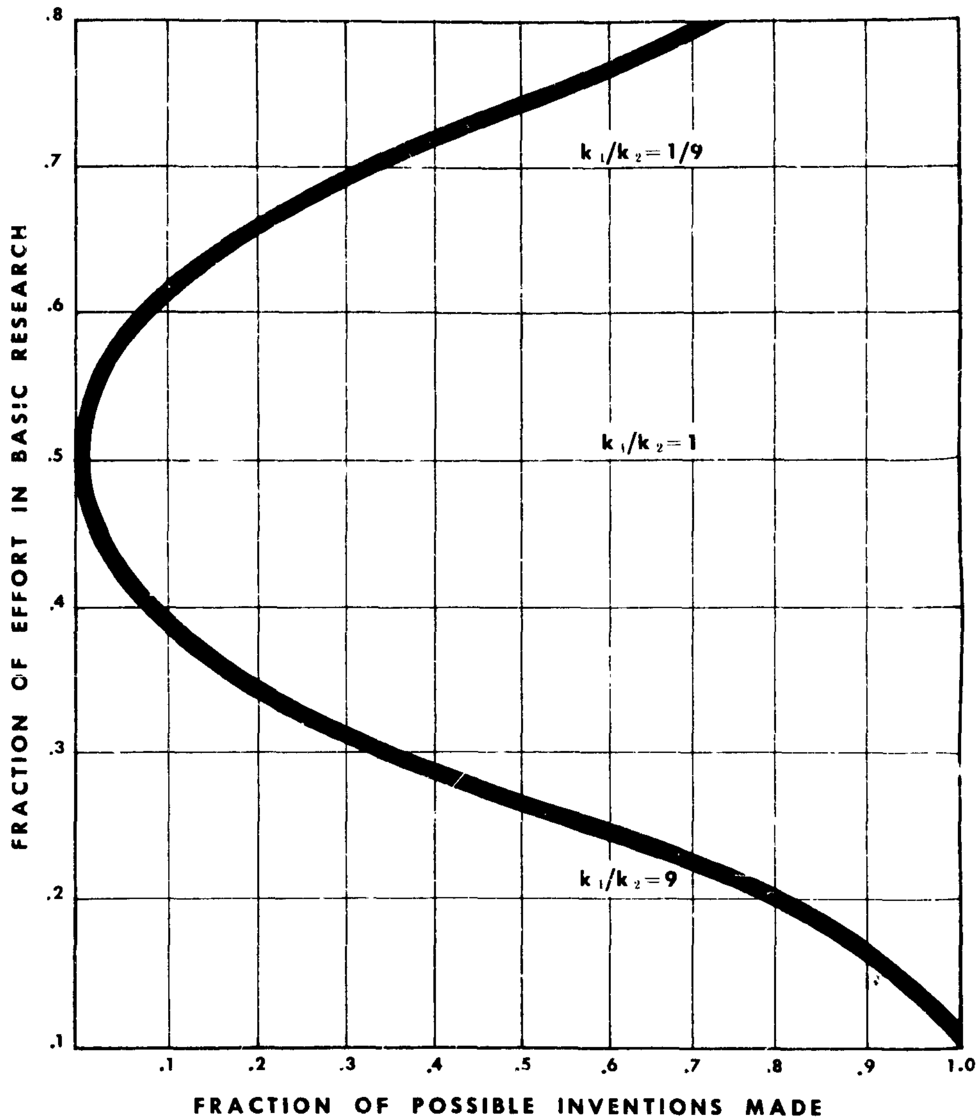
A TYPICAL HISTORY OF THE RESEARCH PROCESS

Figure 12



DEPENDENCE OF RESEARCH RESULTS ON DISTRIBUTION OF EFFORT

Figure 13



FRACTION OF EFFORT IN BASIC RESEARCH AS A FUNCTION OF THE DEGREE OF CONVERSION OF FACTS TO INVENTIONS

Figure 14